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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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PLATES.

ERRATA.

Page 61, line 6, *for* turnpeg *read* tamping.
line 26, *for* rivets *read* roots.
311, line 2, $\frac{dp}{p} = -\frac{g'}{k} \cdot \frac{dz}{1+\alpha\theta}$ (3.)
14, *for* conc. log. *read* com. log.
16, 17, *for* A' *read* A.
at bottom, place × *after* 2077·98.
445, line 8 from the bottom, *for* Pl. I. *read* Pl. III.
V. XIV.
514, line 12, *for* turf *read* tufa.

THE
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[THIRD SERIES.]

JULY 1839.

I. *Note on the Velocity of Sound. By Professor* MILLER, *M.A., F.R.S., F.G.S., &c.**

St. John's Coll., Cambridge, June 12, 1839.

IN the discussion of Moll's experiments on the velocity of sound, by Dr. Simons (Phil. Trans. 1830, page 211) there are two small arithmetical errors. The observations (9.) and (26.), made on the 27th of June 1823, give respectively 51·895 sec. and 51·985 sec. instead of 51·845 and 51·335 as stated by Dr. Simons. The correct mean value of the time in which sound travelled 17669·28 metres is 51·9873 seconds. The mean temperature during these observations was 11°·01 C. The mean pressures of the atmosphere and of the vapour contained in it were equal to the pressures of 0·74618 and 0·00889 metres of mercury at 0° C. respectively. It appears from the recent experiments of Rudberg (Poggendorff's *Annalen*, xli. 558; xliv. 119) that the volume of dry air at 100° C. divided by its volume at 0° C. under a constant pressure, is equal to 1·365. Hence, at the temperature T° centigrade, the pressure of the atmosphere and of the vapour contained in it being Π and Υ respectively, the velocity of sound in English feet per second will be

$$1090{\cdot}77 \sqrt{\left\{\frac{1+0{\cdot}00365\ \mathrm{T}}{1-0{\cdot}375\,\frac{\Upsilon}{\Pi}}\right\}}.$$

* Communicated by the Author.

II. *Notices in Analytical Chemistry.* *By* Mr. T. Richardson*.

1. *Notice of an Analysis of the Sesquichloride of Carbon.*

THIS substance was formed in the usual way by passing a current of pure chlorine gas through the liquid of the Dutch chemists, till a sufficient quantity was obtained for analysis. The crystalline substance obtained was at first well washed with distilled water, and after repeated crystallization from different solutions in alcohol, was considered pure. In this state it had scarcely any taste, an odour similar to that of camphor, and possessed, in short, all the characters of pure sesquichloride of carbon.

Analysed in the usual way by means of Liebig's apparatus, ·772 gram. of the crystals dried at 300° Fahrenheit, gave ·291 grm. CO_2, and ·006 H_2O grm. water. The small quantity of hydrogen evidently arises from hygrometric moisture, and allowing the deficiency to be chlorine, we have

Carbon.........................	10·42
Chlorine	89·58
	100·00

which is equivalent to

1 atom carbon......	76·44	10·30
3 atoms chlorine...	663·96	89·70
	740·40	100·00

2. *On the Employment of Chromate of Lead in the Analysis of Organic Substances* †.

The chromate of lead may be prepared by mixing a salt of lead with bichromate of potash, and carefully washing the precipitated salt. The pure salt, when perfectly dry, is to be heated in a clay crucible till it melts. When well melted it does not absorb so much moisture, and possesses in this respect a great advantage over the oxide of copper. Before employing the salt in analysis it should be finely pounded, and afterwards placed for a short time in a warm place to expel any hygrometric moisture. The mixture with the organic body to be analysed, is made in precisely the same way as with oxide of copper, only that it ought to be as intimate as possible, since a larger portion of substance becomes exposed to the action of the heat in the same time, than with the oxide of copper. The length of the tube necessary for combustion is about 10 inches long and 4ths of an inch in

* Communicated by the Author.

† From the Transactions of the Nat. Hist. Society of Northumberland, Durham, and Newcastle-upon-Tyne, vol. ii. p. 412.

diameter. It is almost unnecessary to add, that the combustion must be very slowly conducted.

During the whole of the analysis, a quantity of oxygen gas is disengaged from the potash apparatus, which arises from the great predisposition of the chromate of lead to be converted into a basic salt. This fact, with the great quantity of oxygen which the salt contains, renders it very advantageous in the combustion of those bodies which contain a large quantity of carbon, and are difficult to consume. With this salt a much larger quantity of substance can be submitted to analysis than with oxide of copper, arising from its greater specific gravity. It is also an excellent means of analysing bodies containing chlorine, bromine, &c., the chloride, bromide, &c. of lead not being volatile.

For the suggestion of the employment of this body in organic analysis we are indebted to Prof. Liebig.

The following analysis was made with the view of testing its accuracy: ·8166 grm. of ordinary sugar, gave 1·241 grm. CO_2, and ·4725 grm. H_2O, which produces in 100 parts,

		Theory.
Carbon.........	42·02	42·403
Hydrogen......	6·40	6·390
Oxygen.........	51·58	51·207
	100·00	100·00

III. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions.* *By* James Ivory, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from vol. xiv. p. 352.]

6. IN the paper published in the Philosophical Transactions for 1823, the refractions are deduced entirely from this very simple formula,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1 - c^{-u}), \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4.)$$

in which β stands for the dilatation of air, or a gas, by heat; τ' is the temperature at the earth's surface, and τ the temperature at any height above the earth's surface; at the same height c^{-u} is the density of the air in parts of its density at the surface.

In order to understand the application of the formula, it is necessary to premise that in the remaining part of this paper we do not consider a variable atmosphere subject to continual fluctuations, as is the case of the real atmosphere: we con-

template an atmosphere fixed in its condition at any given place or observatory, being supposed a mean between all the variations that actually take place in an indefinite time. In such an atmosphere the temperature and pressure at the earth's surface will be mean quantities deduced from observation: the air at all elevations will have an elastic force equal to the incumbent weight which it supports, as an equilibrium requires: and, whether the air be dry or moist, its refractive power will be equal to the refractive power of dry air subjected to the same pressure and temperature *. These properties of the mean atmosphere rest upon experiment and demonstration: in other respects its nature is not directly known to us: and the laws of its action can only be discovered, not by hypothesis, but by observation.

The consideration of a mean atmosphere, invariable at any given observatory, is a necessary consequence of the notion we attach to the mean refractions; for these would be realized in such an atmosphere: but they are different in any other state of the air.

These observations being premised, if the formula (4.) be verified at the earth's surface in any invariable atmosphere, by giving a proper value to the constant f, it will still hold, at least with a very small deviation from exactness, at a great elevation, probably at a greater elevation than has ever been reached by man. In order to prove this, let the arbitrary function $\phi(u)$ be added, so as to complete the formula by rendering it perfectly exact: then

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1-c^{-u}) - \phi(u), \quad \dots \quad \dots \quad (5.)$$

and it will follow that $\phi(u) = 0$, when $u = 0$, that is, at the earth's surface. Again, differentiate the equation, observing that τ decreases when u increases, then

$$\frac{\beta}{1+\beta\tau_{\prime}} \cdot \frac{d\tau}{du} = fc^{-u} + \frac{d \cdot \phi(u)}{du}:$$

now, since this equation is true for all values of u, it will hold at the earth's surface, or when $u = 0$: and if f be taken equal to the particular value of

$$\frac{\beta}{1+\beta\tau'} \cdot \frac{du}{d\tau},$$

when $u = 0$, it will follow that $\frac{d \cdot \phi(u)}{du} = 0$, when $u = 0$.

And since the equations $\phi(u) = 0$ and $\frac{d \cdot \phi(u)}{du} = 0$, are both

* *Additions à la Conn. des Temps*, 1839, p. 36.

verified at the earth's surface, it follows that the supplementary function $\phi\ (u)$ will vary slowly as u increases, that is, as the density of the air decreases in ascending. This proves that the approximate equation (4.) will be very little different from the exact equation (5.) at great elevations in the atmosphere.

At the surface of the earth $d\,u$ is equal to the variation of the density for a depression of the thermometer expressed by $d\,\tau$: and although the proportion of these two quantities cannot be ascertained by direct experiment, yet, as is shown in the paper of 1823, it is easily deduced from the rate at which the temperature decreases as the height increases, which rate is easily determined experimentally. The quantity f thus found is necessarily constant at the same observatory. It is the mean of all the occasional values, which vary incessantly, while the real atmosphere undergoes every vicissitude of which it is susceptible. The mean refraction and f are invariable in quantity, because they depend alike upon the mean condition of the air at a given place. Some confusion has arisen on this point from not distinguishing between the mean refraction of a star and its true refraction in a variable atmosphere.

In all that has been said there is no supposition of an arbitrary constitution of the atmosphere. The assumed formula (4.) is an approximate truth in every invariable state of the air in equilibrium. Conceive a cylindrical column of air perpendicular to the earth's surface, and extending to the top of the atmosphere; at the height where the temperature is τ, and the density ϱ, let p denote the weight of the column above the height; and suppose that p, ϱ, τ are changed into p', ϱ', τ' at the surface of the earth; because the pressures are proportional to the elasticities, we have the usual equation,

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times \frac{\varrho}{\varrho'}:$$

or, which is the same,

$$c^{-u} = \frac{\varrho}{\varrho'},\ \frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times c^{-u}:$$

and by substituting the complete expression of the temperature as given in (5.), we shall obtain,

$$\frac{p}{p'} = c^{-u} - f\,(c^{-u} - c^{-2u}) - c^{-u} \times \phi\ (u): \qquad (6.)$$

and if we omit the supplemental part, which is small even at great elevations, the result will be,

$$\frac{p}{p'} = c^{-u} - f\,(c^{-u} - c^{-2u}).$$

Now this is the constitution of the atmosphere in the paper of 1823; it is only approximate; but it is an approximation applicable to every atmosphere that can be imagined, requiring no more than that the quantity f have the proper experimental value given to it. It is shown in the paper that the pressures and densities are thus represented with no small degree of accuracy at the greatest heights that have been reached; which proves how slowly the supplemental part of the formula (5.) comes into play.

The foregoing manner of arriving at the constitution of the atmosphere adopted in the paper of 1823, being drawn from properties immediately applicable to the problem in hand, is more natural, and more likely to suggest itself, and more satisfactory than the ingenious and far-fetched procedure of M. Biot, of transforming an algebraic formula for the express purpose of bringing out a given result. Laplace, having remarked that the true horizontal refraction is contained between the like quantities of two atmospheres, with densities decreasing in arithmetical and geometrical progression, conjectured that an atmosphere between the two limits, which should likewise agree with observation at the horizon, would in all probability represent the mean refractions with considerable accuracy. It is upon this assumption that the problem is solved in the *Mec. Celeste*, the observed horizontal refraction being used for determining the arbitrary constant. Now in the paper of 1823 there is no allusion to interpolating an atmosphere between two others; a knowledge of the horizontal refraction is not required; the investigation is grounded upon a property common to every atmosphere in a quiescent state; and lastly, the resulting table is essentially different from all the tables computed by other methods. If these considerations be not sufficient to stamp an appropriate character upon the solution of a problem, it would be difficult to find out what will be sufficient. But if it be possible, with M. Biot's ingenuity, to trace some relation in respect of the algebraic expressions, between the paper of 1823 and the calculations of Laplace, from which, after all, no just inference can be drawn, it is not difficult to find between the same paper and the view of the problem taken by the author of the Principia, in 1696, an analogy much more simple and striking, which deserves to be mentioned as it tends to bring back the investigation to the right tract, which it seems to have left. Newton, having solved the problem on the supposition that the density of the air is produced solely by pressure, found that the refractions thus obtained greatly exceeded the observed quantities near the horizon: and hence he inferred, in

the true spirit of research, that there must be some cause not taken into account, such as the agency of heat, which should produce, in the lower part of the atmosphere, the proper degree of rarefaction necessary to reconcile the theoretical with the observed refractions. Now, in the paper of 1823, the sole intention of introducing the quantity f, not noticed before by any geometer, is to cause the heat at the earth's surface to decrease in ascending at the same rate that actually prevails in nature; which evidently has the effect of supplying the desideratum of Newton.

The remarks that have just been made are not called for by anything which M. Biot has written in his dissertation on the refractions, inserted in the additions to the *Conn. des Temps* for 1839; because that author has fully explained the grounds of what he advances, thereby enabling a candid inquirer to form his own opinion: but all the world are not of the same character as that distinguished philosopher.

At every point on the earth's surface we are now acquainted with three things, not hypothetical or precarious, that have an influence on the mean refractions. These are, the refractive power of the air, the spherical figure of the atmosphere, and the mean rate at which the density of the air decreases at the given place. These three things are independent on one another, and on all other properties of the air: they will therefore produce three independent parts of the quantity sought. The parts thus determined may fall short of the whole refraction at any altitude, because there may be causes not taken into account that co-operate in producing the result: but each will unalterably maintain its proper share of the total amount, in whatever way it is attempted to solve the problem, provided the solution is conducted on right principles and not warped by arbitrary suppositions. It may therefore be said that, in so far, an advance has been made in acquiring an exact notion of the nature of this problem.

The table in the paper of 1823 was compared with the best observations that could be procured at the time of publication; and the results were very satisfactory. After the publication of the *Tabulæ Regiomontanæ*, it was found that the table agreed with Bessel's observed refractions to the distance of 88° from the zenith, which is as far as his determinations can be depended on, with such small discrepancies as may be supposed to exist in the observations themselves. So close an agreement between the theoretical and observed mean refractions was very unexpected, and even contrary to the opinion very generally held on this subject.

Astronomers are in the habit of using different tables or

formulas of refraction, which, being derived from conjectural views, do not agree with one another, except to a limited distance from the zenith. Now this is contrary to the very conception we have of the mean refractions, which are determinate and invariable numbers, at least at the same observatory. A great advantage would therefore ensue from setting aside every uncertain table, and substituting in its place one deduced from the causes really existing in nature that produce the phænomena. Such a table adapted to every observatory, if this were found necessary, would contribute to the advancement of astronomy by rendering the observations made at different places more accurately comparable. It might contribute to the advancement of knowledge in another respect: for if the mean refractions were accurately settled, the uncertainty in the place of a star would fall upon the occasional corrections depending on the indications of the meteorological instruments; and it is not unreasonable to expect that much which is at present obscure and perplexing on this head might be cleared up, if it were separated from all foreign irregularities, and made the subject of the undivided attention of observers.

7. The paper in the Philosophical Transactions for 1823 takes into account only the rate at which the densities in a mean atmosphere vary at the surface of the earth; what follows is an attempt to complete the solution of the problem by estimating the effect of all the quantities on which the density at any height depends. For this purpose it will be requisite to employ certain functions of a particular kind, viz.

$$R_1 = 1 - c^{-u},$$

$$R_2 = 1 - u - c^{-u},$$

$$R_3 = 1 - u + \frac{u^2}{1.2} - c^{-u},$$

$$\vdots$$

$$R_i = \left(1 - u + \frac{u^2}{1.2} - \frac{u^3}{1.2.3} \dots \pm \frac{u^{i-1}}{1.2.3\dots i-1}\right) - c^{-u}.$$

In these expressions c is the number of which the hyperbolic logarithm is unit; and it is obvious that R_i is zero when $u = 0$. These expressions have several remarkable properties, which are proved by merely performing the operations indicated.

1st. $$\frac{d \,.\, R_i}{du} = - R_{i-1},$$

$$\int - R_i \, du = R_{i+1},$$

the integral being taken equal to zero, when $u = 0$.

2ndly.
$$\frac{d \,.\, c^{-u} R_i}{c^{-u} \,.\, du} = -(R_{i-1} + R_i),$$

$$\frac{dd \,.\, c^{-u} R_i}{c^{-u} \,.\, du^2} = R_{i-2} + 2R_{i-1} + R_i$$

$$\frac{d^3 \,.\, c^{-u} R_i}{c^{-u} \, du^3} = -(R_{i-3} + 3R_{i-2} + 3R_{i-1} + R_i),$$

&c.

3rdly, n being less than i,

$$\int \frac{d^n \,.\, c^{-u} R_i}{c^{-u} \,.\, du^n} du = (-1)^n \,.\, \frac{d^n \,.\, c^{-u} R_{i+1}}{c^{-u} \,.\, du^n}.$$

These things being premised, the temperature of an atmosphere in equilibrium will have for its complete expression this formula,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f R_1 - f' \frac{d \,.\, c^{-u} R_3}{c^{-u} \,.\, du} - f'' \,.\, \frac{d^2 \,.\, c^{-u} R^5}{c^{-u} \,.\, du^2} - \&c. \quad (7.)$$

the coefficients f, f', f'', &c. being indeterminate constant quantities. A little attention will show that this expression is equivalent to a series of the powers of u; for, first, let the differential operations in the several terms be performed, which will bring out

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f R_1 + f(R_2 + R_3) - f''(R_3 + 2R_4 + R_5) + \&c.;$$

next, expand R_1, R_2, &c., and the result will be,

$$\begin{aligned} \frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu &+ (f - f') \,.\, \frac{u^2}{1.2} \\ &- (f - 2f' + f'') \,.\, \frac{u^3}{1.2.3} \\ &+ (f - 2f' + 3f'' - f''') \frac{u^4}{1.2.3.4} \\ &- \&c. \end{aligned}$$

The intention of assuming the formula (7.) is to express the temperature in terms of such a form as will produce, in the refraction, independent parts that decrease rapidly.

In order to elucidate what is said, and more especially to prove that the analysis here followed comprehends all atmospheres, whether of dry air or of air mixed with aqueous vapour; let p', ϱ', τ' denote, as before, the pressure, the density, and the temperature, at the surface of the earth; and put p, ϱ, τ for the like quantities at the elevation z above the sur-

face: the equations of equilibrium are these two, the radius of the earth being represented by a, viz.

$$p = \int \frac{-dz \cdot \rho}{\left(1+\frac{z}{a}\right)^2},$$

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \cdot \frac{\rho}{\rho'}.$$

The second of these equations has already been noticed; the integral in the first being extended to the top of the atmosphere, is equal to the weight of the column of air above the initial height, every infinitesimal mass being urged by a gravitation which is equal to unit at the earth's surface, and decreases in the inverse proportion of the square of the distance from the earth's centre. By putting

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1-q, \quad \sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{\rho}{\rho'} = c^{-u},$$

the same two equations will be thus written, viz.

$$p = \rho' \int - d\sigma\, c^{-u},$$
$$p = p'(1-q)\, c^{-u}.$$

The three quantities u, q, σ, are severally equal to zero at the earth's surface: and the two values of p will not be identical, unless the same three quantities can be expressed by functions of one variable, or, which is equivalent, unless two of them, as q and σ, are each functions of the remaining one u. Now q being a function of u, we shall have,

$$q = \frac{dq}{du} \cdot u + \frac{ddq}{du^2} \cdot \frac{u^2}{1.2} + \frac{d^3q}{du^3} \cdot \frac{u^3}{1.2.3}, \text{ \&c.}$$

the differentials being valued when $u = 0$, that is, the particular values which they have at the earth's surface being taken. According to what was before shown, we have this other series for q, viz.

$$q = fu - (f-f') \cdot \frac{u^2}{1.2} + (f-2f'+f'') \cdot \frac{u^3}{1.2.3} \text{ \&c.}:$$

and as the two series must be identical, it follows that the quantities f, f', f'', &c., will be known, if we can ascertain the particular values assumed at the surface of the earth by the differentials of q considered as varying with u, or with the density. Thus the coefficients in the formula (7.) are not hypothetical quantities, but such as have a real existence in nature, and which might be determined experimentally, if we had the means of observing the phænomena of the atmosphere

with sufficient exactness, so as to be able to determine q when u is given. It is further to be observed, that the same formula is general for all atmospheres, whether the air be entirely dry, or mixed with aqueous vapour: for it has been investigated from equations common to all atmospheres in equilibrium, without any consideration of a particular state of the air.

By substituting the series for q in the equation

$$\frac{p}{p'} = (1-q)\, c^{-u},$$

we obtain,

$$\frac{p}{p'} = c^{-u} - f c^{-u} \mathrm{R}_1 - f' \frac{d \,.\, c^{-u} \mathrm{R}_3}{d u} - f'' . \frac{d \,.\, c^{-u} \mathrm{R}^5}{d u^2} - \&\text{c.} \quad (8.)$$

Further, if this value of $\frac{p}{p'}$ be substituted in the equation

$$\frac{p}{p'} = \frac{\rho'}{p'} \int - d\sigma\, c^{-u},$$

we shall find

$$\int - d\sigma\, c^{-u} = \frac{p'}{\rho'} . \left\{ c^{-u} - f . c^{-u} \mathrm{R}_1 - f' \frac{d \,.\, c^{-u} \mathrm{R}_3}{d u} - \&\text{c.} \right\}.$$

Now, let this expression be differentiated; then divided by c^{-u}; and, finally, integrated, attending to the nature of the functions concerned; and the following result will be obtained:

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \frac{p'}{\rho'} \left(u - f . \frac{d . c^{-u} \mathrm{R}_2}{c^{-u} . d u} - f' \frac{d d . c^{-u} \mathrm{R}_4}{c^{-u} . d u_2} - \&\text{c.} \right) (9.)$$

The equations (7.), (8.), (9.) contain the theoretical explanation of the properties of the atmosphere. What is said may easily be proved by applying them to such phænomena as have been ascertained in a satisfactory manner. This application is besides necessary for determining the numerical values of the coefficients f, f', f'', &c., which enter into the expression of the refraction. For this purpose it is requisite to find the relations that subsist between the pressure, the temperature, and the height above the earth's surface, by combining the equations so as to exterminate u.

By performing the differentiations in the equation (9.), there will be obtained,

$$\sigma = \frac{p}{\rho} \{ u + f(\mathrm{R}_1 + \mathrm{R}_2) - f'(\mathrm{R}_2 + 2\mathrm{R}_3 + \mathrm{R}_4) + \&\text{c.} :$$

and, by expanding the functions,

$$\sigma = \frac{p'}{\rho'} . \left\{ (1+f)\, u - (2f - f') . \frac{u^2}{2} + (2f - 3f' + f'') \frac{u^3}{1.2.3} - \&\text{c.} \right.$$

Now, by reverting the series for q, we get

$$u = \frac{q}{f} + \frac{f-f'}{2f} \cdot \frac{q^2}{f^2} + \frac{2f^2-4ff'+3f'^2-ff''}{6f^3} \cdot \frac{q^3}{f^3} + \&c. ;$$

and, by substituting this value of u, the following formula will be obtained:

$$\sigma = \frac{z}{1+\frac{z}{a}} = \frac{p'}{\rho'} \cdot \left(\frac{1+f}{f} \cdot q + \frac{f-f'-f^2}{2f^3} \cdot q^2 + \&c. \right) \ldots \text{(A.)}$$

This equation between the perpendicular elevation z, and the difference of temperature

$$q = \frac{\beta(\tau'-\tau)}{1+\beta\tau'},$$

contains the law according to which the heat decreases as the height above the earth's surface increases.

Further, from the equation

$$\frac{p}{p'} = (1-q)\, c^{-u},$$

we deduce

$$\log\left(\frac{p'}{p}\right) = u + \log\frac{1}{1-q} = u + q + \frac{q^2}{2} + \frac{q^3}{3} + \&c.$$

and, by substituting the value of q,

$$\log\frac{p'}{p} = (1+f)\,u - \frac{f-f'-f^2}{2}\,u^2$$
$$+ \frac{f-2f'+f''-3f^2+3ff'+2f^3}{6}\,u^3, \&c.$$

By means of this series and the value of σ in terms of u already found, it is easy to deduce

$$\sigma = \log\left(\frac{p'}{p}\right) \times \frac{p'}{\rho'} \left(1 - \frac{f}{2} \cdot u\right.$$
$$\left. + \frac{2f-2f'+3f^2-3ff'-f^3}{12(1+f)} \cdot u^2 - \&c.\right);$$

and, by substituting the value of u, we finally obtain

$$\sigma = \frac{z}{1+\frac{z}{a}} = \log\frac{p'}{p} \times \frac{p'}{\rho'} \cdot \left(1 - \frac{1}{2}\,q - \frac{f+f^3-f'}{12f^2(1+f)} \cdot q^2, \&c.\right) \text{(B.)}$$

This formula determines a perpendicular ascent z, when the difference of the pressures, and of the temperatures, at its upper and lower extremities, have been found.

The formulas that have been investigated are true in an atmosphere of air mixed with aqueous vapour, as well as in one of perfectly dry air; but in applying them, perspicuity requires that the two cases be separately considered.

[To be continued.]

IV. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from vol. xiv. p. 184.]

Barranquilla; village on the river Magdalena about six leagues from the coast.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1820.				
Aug. 5.	89°·50	1 p.m.	0	Weather bright and close with storms in the evening; frequent play of lightning on the horizon.
6.	87	11 a.m.	,,	
7.	85	2 p.m.	,,	
8.	80	6 a.m.	,,	
,,	87	12	,,	
,,	83	5 p.m.	,,	
9.	79	6 a.m.	,,	
,,	89	1 p.m.	,,	
,,	85·50	4 p.m.	,,	
25.	75	8 a.m.	,,	Heavy rain.
,,	76·50	1 p.m.	,,	Fair.
,,	79	4	,,	Do.
26.	85	1 p.m.	,,	Cloudy.
,,	80	5	,,	Do. Storm at night.
27.	85	12	,,	Cloudy.
,,	81	2 p.m.	,,	Storm.
,,	78	6	,,	
28.	77	6 a.m.	,,	Fair.
,,	83	3 p.m.	,,	Do.
29.	87	12	,,	Do.
30.	79	6 a.m.	,,	
,,	86	12	,,	
,,	88	2 p.m.	,,	Bright.
11 days. Average maximum 85°·63. Minimum of five days 78°. Medium 81°·81.				
Sept. 1.	81	6 a.m.	,,	Bright.
,,	88	2 p.m.	,,	
2.	82	6 a.m.	,,	
,,	79	8	,,	Rain.
,,	84	12	,,	Fair.
3.	78	6 a.m.	,,	Do.
,,	86	3 p.m.	,,	
4.	80	6 a.m.	,,	Do.
,,	87	11	,,	
5.	78	6 a.m.	,,	Do.
,,	88	12	,,	
6.	78	6 a.m.	,,	Cloudy.
,,	84	12	,,	

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1820.			0	
Sept. 7.	75°	7 a.m.	„	Cloudy.
„	83	12	„	
8.	72	6 a.m.	„	Rain, and thunder at night.
„	85	12	„	
9.	87	12	„	Bright.
10.	86	6 a.m.	„	Fair.
„	85	12	„	
11.	77	6 a.m.	„	Do.
„	88	2 p.m.	„	
12.	77	6 a.m.	„	Do.
„	87	12	„	Cloudy.
„	79	2 p.m.	„	Storm.
13.	77	6 a.m.	„	Fair.
„	87	12	„	
14.	81	7 a.m.	„	
„	80·50	2 p.m.	„	Rain.
15.	75	6 a.m.	„	Showery.
„	83	1 p.m.	„	
16.	77	6 a.m.	„	Cloudy.
„	83	12	„	
17.	85	4 p.m.	„	Fine.
18.	80	6 a.m.	„	Do.
„	90	12	„	
„	84	5 p.m.	„	
19.	78	6 a.m.	„	Fine.
„	90	1 p.m.	„	
20.	81	7 a.m.	„	Do.
„	90	12	„	
21.	80	6 a.m.	„	Do.
„	92	9 a.m.	„	
„	87	3 p.m.	„	
22.	84	7 a.m.	„	Do.
„	89	12	„	
23.	80	7 a.m.	„	Do.
„	89	1 p.m.	„	
„	82	8	„	
24.	77	7 a.m.	„	Foggy.
„	89	12	„	Fair.
25.	80	6 a.m.	„	
„	89	1 p.m.	„	
„	77	do.	„	Storm.
26.	77	6 a.m.	„	Fine.
„	87·50	12	„	
27.	82	7 a.m.	„	Do.
„	90·50	1 p.m.	„	
28.	81	6 a.m.	„	Do.
„	87	12	„	
29.	87·50	1 p.m.	„	Do.
30.	88	12	„	Do.

30 days. Average maximum 87°·26. Minimum 79°·66. Medium 83°·46.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1820.				
Oct. 1.	92	12	0	Fine.
2.	86	3 p.m.	,,	Do.
3.	80	6 a.m.	,,	
,,	88	11	,,	Do.
,,	86	1 p.m.	,,	
4.	79	7 a.m.	,,	Fair.
,,	86	1 p.m.	,,	Do.
5.	80	7 a.m.	,,	Fair.
,,	84	3 p.m.	,,	
6.	75	6 a.m.	,,	Cloudy.
,,	86	3 p.m.	,,	
7.	75	7 a.m.	,,	Do. rain at night.
,,	78	7 p.m.	,,	
8.	76	7 a.m.	,,	Fine.
,,	86	12	,,	
9.	77	6 a.m.	,,	Cloudy.
,,	87	1 p.m.	,,	
10.	80	9 a.m.	,,	Do.
,,	83	12	,,	
11.	85	12	,,	Fine.
12.	78·50	7 a.m.	,,	
,,	85	12	,,	
,,	79	3 p.m.	,,	Rain.
13.	75	7 a.m.	,,	
,,	86	2 p.m.	,,	
,,	76	5 p.m.	,,	Rain.

13 days. 85°·53 maxima, 77°·16 minima } Med. 81°·35.

Average of 54 days 82°·21.

City of Santa Marta. Lat. 11° 19′ 2″ *N. situated on the Coast, a few miles from the base of the Snowy Mountains.*

Nov. 20.	79	7 a.m.	0	
,,	83·50	12	,,	Fine.
21.	79	7 a.m.	,,	Do.
,,	84	11	,,	
22.	79	7 a.m.	,,	Fine.
,,	85	12	,,	Do.
23.	do.	do.	,,	
24.	78	7 a.m.	,,	Fine.
,,	85	12	,,	
25.	id.	id.	,,	id.
26.	78	7 a.m.	,,	id.
,,	84	2 p.m.	,,	
27.	id.	id.	,,	id.
28.	79·50	7 a.m.	,,	High winds.
,,	85	1 p.m.	,,	

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1820.	°			
Nov. 29.	80	7 a.m.	,,	High winds.
,,	85	1 p.m.	,,	
30.	79	7 a.m.	,,	Fair.
,,	85	1 p.m.	,,	
11 days 84°·54 max. 78°·72 min — 81°·63 med.				
Dec. 1.	79	7 a.m.	,,	Fine.
,,	84·50	8 p.m.	,,	
2.	82	7 a.m.	,,	Id.
,,	86	12	,,	
3rd to 6th	id.	id.	,,	Id.
7.	79	7 a.m.	,,	Id.
,,	85·50	1 p.m.	,,	
8.	80	7 a.m.	,,	Id.
,,	85	4 p.m.	,,	
9.	80·50	7 a.m.	,,	Id.
,,	86·5	4 p.m.	,,	
10.	80	7 a.m.	,,	Id.
,,	86	2 p.m.	,,	
11.	id.	id.	,,	Id.
12.	id.	id.	,,	Id.
13.	81	7 a.m.	,,	Id.
,,	87	2 p.m.	,,	
14 to 24.	id.	id.	,,	Id.
25.	79	7 a.m.	,,	Id.
25 days 89° max. 80°.62 min. — 83°·81 med.				
1821.				
Jan. 4.	75	6½ a.m.	,,	Fair.
,,	81	12	,,	,,
5.	76	7 a.m.	,,	Id.
,,	83	3 p.m.	,,	
6.	77	7 a.m.	,,	Id.
,,	81·5	12	,,	
7.	79	7 a.m.	,,	Id.
,,	83	12	,,	
8.	79	7 a.m.	,,	Id.
,,	85	4 p.m.	,,	
9.	80	6 a.m.	,,	Much wind.
,,	85	4 p.m.	,,	Id.
10.	80	6 a.m.	,,	
,,	87	4 p.m.	,,	
11.	81	7 a.m.	,,	Fair.
,,	86	1 p.m.	,,	
12 and 13.	id.	id.	,,	Id.
14.	74	7 a.m.	,,	Id.
,,	84	3 p.m.	,,	

Table *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821.	°			
Jan. 15.	78	7 a.m.	,,	Fair.
,,	82	12	,,	
16.	78	7 a.m.	,,	Id.
,,	84	1 p.m.	,,	
18 & 19.	id.	id.	,,	Id.
20.	86	1 p.m.	,,	
21.	80	8 a.m.	,,	Id.
,,	86	12	,,	
22.	85	12	,,	Id.
23 to 31.	id.	id.	,,	Id.
26 days 84·6 max., 78·23 min. } 81°·41 med. Average of 62 days 82°·28.				

Village of La Cienaga on the Sea Coast.

Feb. 3.	77·0	7 a.m.	,,	Fair.
4.	id.	id.	,,	Id.

Caño Bovar, wooded plains of the Interior.

5.	67	6 a.m.	,,	Id.
6.	66	6½ a.m.	,,	Id.

Village of La Fundacion, surrounded by Forests.

7.	87	10 a.m.	0, or very trifling.	Fair.
,,	91·5	2 p.m.		
8.	69	6½ a.m.	,,	Fair.
,,	91	2 p.m.	,,	
9.	69	6½ a.m.	,,	Id.
3 days 91°·5 max., 69 ·0 min. } 80°·25 med.				

Village of Los Chimeles, near the base of the Snowy Mountains, Country stripped of Vegetation by heat and drought.

10.	98	1 p.m.	Elevation probably less than 500 feet.	Fair.
,,	102	2 p.m.		
11.	70	8 a.m.		Id.

Village of Guaycaras, like the former.

12.	85	12	Same level.	Fair.

Town of Valencia de Jesus, in the valley of Upar.

Date.	Time.	Thermo-meter.	Elevation.	Remarks.
1821.				
Feb. 14.	90°	1 p.m.	Little ele-	Fair.
,,	91	4 p.m.	vation ;	
15.	76	6 a.m.	lying be-	
	88	12	twixt the	
	91	3 p.m.	Santa	
16 & 17.	id.	id.	Marta &	Id.
18.	82	8 a.m.	Ocaña	Id.
			ridges.	

La Cueva, on the ascent of the Snowy Mountains.

19.	67·5	7 a.m.	Probably 1000 feet.	

Indian Village of San Sebastian in the Mountains.

19.	72	4 p.m.	Probably	
20.	63	7 a.m.	about	
,,	65	8 a.m.	6000 feet.	Fair.
,,	73	1 p.m.		
,,	72	4 p.m.		

Foot of the Snowy Mountains.

22.	22	5½ a.m.	About	
,,	73 sun.	9	15000	
			feet.	

Village of El Venado; scorched plains.

28.	108	1 p.m.	1	Bright.

Village of El Paso, like the former.

Mar. 1.	103	3 p.m.	,,	Id.
2.	83	7 a.m.	,,	Id.
3.	103	1 p.m.	,,	Id.

Town of Chirguana; open plains.

4.	77	7 a.m.	Little	Foggy mornings, and breeze towards midday.
,,	93	2 p.m.	elevation.	
5.	77·5	6 a.m.	,,	
,,	92	1 p.m.	,,	

Village of San Bernardo; wooded plains.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821. Mar. 8.	79°·5	7 a.m.	„	Rain at night.
9.	75	7 a.m.	„	Id.
„	82	2 p.m.	„	
10.	79	7 a.m.	„	Id.

Village of Parroquia del Carmen: Mountain defiles.

12.	69·5	6 a.m.	About 1000 ft.	

City of Ocaña. Lat. 7° 50′, surrounded by heights.

14.	63	6 a.m.	Probably from 2000 to 3000 ft.	Weather constantly fair; sky bright and clear.
„	80	4 p.m.		
15.	64	6½ a.m.		
„	81·5	12		
16.	62	6½ a.m.		
„	83	12		
17.	63	6 a.m.		
„	78	4 p.m.		
18.	62	6 a.m.		
„	78	4 p.m.		
19.	68	6 a.m.		
„	78	4 p.m.		
20.	65	6 a.m.		
„	84	2 p.m.		
21.	54	6 a.m.		
„	85	2 p.m.		
22.	78	7 a.m.		
9 days 80°·88 max. 64 ·33 min. 72°·60 med.				

Banks of the river Jara, Mountains between Ocaña and Cucuta.

25.	56	6 a.m.		

Village of San Pedro.

28.	68	6 a.m.		

Town of Salaza.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821. Mar. 30.	70	7½ a.m.	Elevation about 1200 ft.	Fine.
,,	78	4 p.m.		
31.	78	7½ a.m.		
,,	82	2 p.m.		Id.

Town of San Josè de Cucuta.

April 4.	77	7 a.m.	Little elevation.	Cloudy and showery.
,,	85	12		
,,	85	10 p.m.		
5.	83	7 a.m.		Id.
,,	85	12		
,,	85	10 p.m.		
6.	87	1 p.m.		Id.
,,	85	10 p.m.		
7.	id.	id.		Id.
8.	88	1 p.m.		Id.
5 days 86° max. 80 min. } 83° med.				
10.	78	6 a.m.	Little elevation.	El Rosario de Cucuta.
11.	70	9 a.m.	Ascent of the mountains of Merida.	San Corstoval Mountains.
12.	59	6 a.m.		
13.	62	7½ a.m.	Elevated valley of Merida.	La Grita.
14.	65	6½ a.m.		Bayladores.
15.	68	6 a.m.	Descent of the valley.	Tabical.
16.	78	6 a.m.	Lowest point of the valley.	Plantation of Estanquez, *Cacao.*
,,	83	3 p.m.		

[To be continued.]

V. *On the Boracic Acid Lagoons of Tuscany.* By John Bowring, *LL.D.**

THE borax lagoons of Tuscany are entitled to a detailed description. They are unique in Europe, if not in the world; and their produce is become an article of equal importance to Great Britain as an import, and to Tuscany as an export. They are spread over a surface of about 30 miles, and exhibit from the distance columns of vapour, more or less according to the season of the year and state of the weather, which rise in large volumes among the recesses of the mountains.

As you approach the lagoons, the earth seems to pour out boiling water as if from volcanoes of various sizes, in a variety of soil, but principally of chalk and sand. The heat in the immediate adjacency is intolerable, and you are drenched by the vapour, which impregnates the atmosphere with a strong and somewhat sulphurous smell. The whole scene is one of terrible violence and confusion—the noisy outbreak of the boiling element—the rugged and agitated surface—the volumes of vapour—the impregnated atmosphere—the rush of waters—among bleak and solitary mountains.

The ground, which burns and shakes beneath your feet, is covered with beautiful crystallizations of sulphur and other minerals. Its character beneath the surface at Mount Cerbole is that of a black marl streaked with chalk, giving it, at a short distance, the appearance of variegated marble.

Formerly the place was regarded by the peasants as the entrance of hell, a superstition derived no doubt from very ancient times, for the principal of the lagoons and the neighbouring volcano still bear the name of Monte Cerboli (*Mons Cerberi*). The peasantry never passed by the spot without terror, counting their beads, and praying for the protection of the Virgin.

The borax lagoons have been brought into their present profitable action within a very few years. Scattered over an extensive district, they are become the property of an active individual, M. Larderel, to whom they are a source of wealth, more valuable perhaps, and certainly less capricious, than any mine of silver that Mexico or Peru possesses. The process of manufacture is simple, and is effected by those instruments which the localities themselves present. The soffioni, or vapours, break forth violently in different parts of the mountain recesses. They only produce boracic acid when they burst with a fierce explosion. In these spots artificial lagoons are

* From Dr. Bowring's Report on the Statistics of Tuscany.

formed by the introduction of the mountain streams. The hot vapour keeps the water perpetually in boiling ebullition; and after it has received its impregnation during twenty-four hours at the most elevated lagoon, the contents are allowed to descend to the second lagoon, where a second impregnation takes place, and then to the third, and so forth, till it reaches the lowest receptacle; and having thus passed through from six to eight lagoons, it has gathered one half per cent. of the boracic acid. It is then transferred to the reservoirs, from whence, after a few hours rest, it is conveyed to the evaporating pans, where the hot vapour concentrates the strength of the acid by passing under shallow leaden vessels from the boiling fountains above, which is quite at a heat of 80° of Reaumur*, and is discharged at a heat of 60°†. There are from ten to twenty pans, in each of which the concentration becomes greater at every descent till it passes to the crystallizing vessels, from whence it is carried to the drying rooms, where, after two or three hours, it becomes ready to be packed for exportation.

The number of establishments is nine‡. The whole amount produced varies from 7000 to 8000 pounds (of 12 ounces) per day. The produce does not appear susceptible of much extension, as the whole of the water is turned to account; the atmosphere has, however, some influence on the result. In bright and clear weather, whether in winter or summer, the vapours are less dense, but the depositions of boracic acid in the lagoons are greater. Increased vapours indicate unfavourable change of weather, and the lagoons are infallible barometers to the neighbourhood, even at a great distance, serving to regulate the proceedings of the peasantry in their agricultural pursuits.

It had been long supposed that the boracic acid was not to be found in the vapours of the lagoons; and when it is seen how small the proportion of acid must originally be, it will not be wondered at that its presence should have escaped attention. In the lowest of the lagoons, after five, six, and in some cases a greater number of impregnations, the quantity of boracic acid given out does not exceed one half per cent.: thus if the produce be estimated at 7500 pounds per day, the quantity of saturated water daily discharged is a million and a half of Tuscan pounds, or five hundred tons English.

The lagoons are ordinarily excavated by the mountaineers of Lombardy, who emigrate into Tuscany during the winter

* The boiling point. † 167° of Fahrenheit.

‡ The principal are Monte Cerboli, Monte Rotondo, Susso, Serazzano, and Castelnuovo.

season, when their native Apennines are covered with snow. They gain about one Tuscan lira per day. But the works are conducted, when in operation, by natives, all of whom are married, and who occupy houses attached to the evaporating-pans. They wear a common uniform, and their health is generally good.

A great improvement in the cultivation, and a great increase in the value, of the neighbouring soil has naturally followed the introduction of the manufacture of the boracic acid. A rise of wages has accompanied the new demand for labour; much land has been brought into cultivation by new directions given to the streams of smaller rivers. Before the boracic lakes were turned to profitable account, their fetid smell—their frightful appearance, agitating the earth around them by the ceaseless explosions of boiling water, and not less the terrors with which superstition invested them*, made the lagoons themselves to be regarded as public nuisances, and gave to the surrounding country a character which alienated all attempts at improvement.

Nor were the lagoons without real and positive dangers, for the loss of life was certain where man or beast had the misfortune to fall into any of those boiling baths. Cases frequently occurred in which cattle perished; and one chemist, of considerable eminence, met with a horrible death by being precipitated into one of the lagoons. Legs were not unfrequently lost by a false step into the smaller pits (*putizze*), where, before the foot could be withdrawn the flesh would be separated from the bone.

That these lagoons, now a source of immense revenue, should have remained for ages unproductive; that they should have been so frequently visited by scientific men, to none of whom (for ages at least) did the thought occur that they contained in them mines of wealth, is a curious phænomenon;

* So unwilling were the peasants to settle in these districts, that very extraordinary encouragements were held out to them. In the commune of Monte Cerboli any inhabitant of the town may sow and reap whatever he pleases without requiring the consent of the owner of the soil; so it frequently happens that small tracts are cultivated which are particularly favoured by water or other advantages, and all the surrounding land left untouched. As the inhabitants have the primary right, the landlord generally abandons his property to the chance cultivation of the peasant, who leaves fallow nine-tenths of the land. In the district of Riparbella the landlords and cultivators have come to a sensible agreement, by apportioning the lands in equal moieties.

Many mineral waters are in the neighbourhood of the lagoons, some of which possess medical virtues, and are visited by the Tuscans in the bathing season.

nor is it less remarkable, that it was left for a man, whose name and occupation are wholly disassociated from science, to convert these fugitive vapours into substantial wealth.

Though to the present proprietor (the Chevalier Larderel*) the merit attaches of having given to the boracic lagoons the immense importance they now possess, a succession of adventurers had made many experiments, and had produced a considerable quantity of boracic acid, but at a cost (from the expenditure of combustible) which left little profit†. The small value that was attached to them may be seen in the fact, that the largest and most productive district of the lagoons, that of Monte Cerboli, was offered in perpetuity, so lately as 1818, at an annual ground-rent of £T. 200*l.* or 6*l.* 13*s.* 4*d.* per annum, though it now produces several thousand pounds sterling. The immense increase in their value arose from the simplest of improvements, the abandonment of the use of charcoal, and the application of the heat of the lagoons or soffioni to the evaporation of their own waters. Improvements, however, and very important ones, particularly by subjecting the waters to a succession of impregnations, had been gradually introduced by a Signor Ciaschi, and the importation of boracic acid from Tuscany into France, before 1817, had been between 7000 and 8000 pounds, of a quality gradually increasing in purity: but Ciaschi perished miserably, in consequence of falling into one of the lagoons which he himself had excavated, leaving his family in a state of extreme poverty. His death (which happened in 1816) naturally threw a damp upon adventure. The experiments were resumed in the following year, and in the midst of violent claims and controversies, M. Larderel has become the monopolist of the boracic productions of Tuscany.

With the increased production of boracic acid has arisen an increased demand, growing out of the more extensive application of it to manufacturing purposes. In about four years the quantity has been quadrupled by superior modes of extraction, and by greater care employed in the collection of the boracic vapour. In 1833 about 650,000 Tuscan pounds were obtained, in 1836 two millions and a half.

* While these sheets have been passing through the press the Grand Duke of Tuscany has conferred on M. Larderel the title of Count de Pomerance.

† Hoefer first announced the presence of boracic acid in the Maremman districts, and Mascagni in his Commentaries suggests the manufacture of borax as an object worthy of attention. Professor Gazzeri in 1807, made experiments, which however seemed to show that the quantity of boracic acid contained in the waters was too small to promise much success.

But it appears to me that the powers and riches of these extraordinary districts remain yet to be fully developed. They exhibit an immense number of mighty steam-engines, furnished by nature at no cost, and applicable to the production of an infinite variety of objects. In the progress of time this vast machinery of heat and force will probably become the moving central point of extensive manufacturing establishments. The steam, which has been so ingeniously applied to the concentration and evaporation of the boracic acid, will probably hereafter, instead of wasting itself in the air, be employed to move huge engines, which will be directed to the infinite variety of production which engages the attention of labouring and intelligent artisans; and thus, in the course of time, there can be little doubt, that these lagoons, which were fled from as objects of danger and terror by uninstructed man, will gather round them a large intelligent population, and become sources of prosperity to innumerable individuals through countless generations.

VI. *The Colours of the Atmosphere considered with reference to a previous Paper "On the Colour of Steam under certain circumstances." By* JAMES D. FORBES, *Esq., F. R. SS. L. & Ed., Professor of Natural Philosophy in the University of Edinburgh.*

[Continued from vol. xiv. p. 426.]

A TOTALLY different hypothesis from any of the preceding, as regards the blue of the sky, was about the same time started by Muncke. He asserts that this hue is, what the German writers call purely *subjective*, that is, an ocular deception, received by the eye on looking into vacant space*. This theory has been well discussed by Brandes, but I think he has not succeeded in explaining Muncke's fundamental experiment, which is this:—If the sky be viewed by one eye directly, and by the other through a long blackened tube, the colour in the latter case gradually seems to vanish. Now, the explanation of this optical difficulty is to be found, I conceive, in the general fact first observed by Mr. Smith†, and which I have verified in a great variety of cases, that when a white object is viewed at once by both eyes, one shaded, and the other powerfully illuminated, though its natural colour is undoubtedly white, it appears *red* to the shaded eye, and *green* to the other. The shaded eye in Muncke's experiment, therefore, superimposes a red impression (by the effect of contrast with

* Schweigger's Journal, xxx. 81; and article *Atmosphäre* in Gehler.
† Edin. Journal of Science, v. 52.

the exposed eye) on the blue which it sees, and being its complementary colour, or nearly so, it must tend to diminish the blueness, and finally to produce white.

Berzelius adopts the view which considers the air itself coloured*.

In the older writings of Sir David Brewster, we find the theory of Bouguer maintained†; but since he has been led to what we must consider, for a majority of cases, a refutation of the Newtonian doctrine of the colours of bodies, he was naturally induced to view with doubt the composition of the celestial blue, and especially of the colours of clouds. That the reflected and transmitted tints should be complementary, as Newton's theory assigns, is well known to be rather the exception than rule in coloured bodies generally; and a very simple prismatic analysis, which it seems difficult to misconstrue, proves that the composition of colours—the green of leaves, for instance,—is widely different from that which the doctrine of thin plates would infer‡. "I have analysed too," he says, "the blue light of the sky, to which the Newtonian theory has been thought peculiarly applicable, but, instead of finding it a blue of the first order, in which the extreme red and extreme violet rays are deficient, while the rest of the spectrum was untouched, I found that it was defective in rays adjacent to some of the fixed lines of Fraunhofer, and that the absorptive action of our atmosphere widened, as it were, these lines. Hence, it is obvious, that there are elements in our atmosphere which exercise a specific action upon rays of definite refrangibility. . . . . I have obtained," he adds, "analogous results in analysing the *yellow*, *orange*, *red*, and *purple* light which is reflected from the clouds at sunset§." Such a prismatic analysis as is here referred to, is even more satisfactory than in the case of the juices of plants, because here the very reflected light itself is examined in the state it reaches the eye. I need hardly add, that this experiment is not less conclusive against the *subjective* theory of Muncke, than against the theory of thin plates of water of Newton and his followers.

Forster, in his treatise on Atmospheric Phænomena, maintains the doctrines of Melvill respecting the colour of clouds. "We observe," he says, "that clouds of the same variety, having the same local or angular position with respect to the

* *Lehrbuch der Chemie*, Wöhler's edit. 1825, i. 346.

† Edin. Encyclopædia, art. *Optics*, p. 620. Compare articles *Atmosphere* and *Cyanometer*.

‡ Life of Newton, p. 78. 1831. Ed. Trans. xii. 538.

§ Ed. Trans. xii. 544. Compare Encyc. Brit. new Edition, art. *Optics*, p. 510.

sun, sometimes appear richly coloured, and at other times scarcely coloured at all,—a circumstance which renders it questionable whether the colour is from the cloud itself, or whether the cloud only reflects the light which is coloured by refraction in passing through the haze of the atmosphere in the evening. The former is, however, probably the case; for different clouds, in nearly the same angular position with respect to the sun, show different colours at the same time*."

I must quote myself as having formerly adopted the theory of Bouguer, with regard at least to the celestial blue. In one of a series of papers on the Bay of Naples, published about ten years ago, I noticed the occurrence of a strictly purple tinge (the poetic *lumen purpureum*), in a perfectly clear sky, which I attributed to a part of the violet rays, mixed with the blue, finding their way to the eye. There is no question (notwithstanding the authority of Eustace†), that Virgil's epithet was founded on the accurate observation of Nature. The fact has also been observed by Humboldt and by Leslie‡. We now come to the theory of M. Leopold Nobili of Reggio, and which, after what has been stated, may be very briefly expounded. In quoting M. Nobili's speculations on this subject as new to me, I must observe, that they are contained in a memoir|| on a certain uniform scale of colours, for the use of artists, produced by the elegant method of depositing thin layers of transparent substances on metallic surfaces, by precipitation from solutions by means of galvanic decomposition. This beautiful art of forming what Nobili calls his "Apparences Electro-chimiques," was first pointed out to me, as well as the papers describing it, by Professor Necker of Geneva, as far back as the winter 1831–2, when some members of the Society may recollect that I exhibited in this room specimens of Nobili's chromatic scale, prepared by myself§.

* Researches about Atmospheric Phænomena, 3rd edit., 1823, p. 86. The continuation of the passage will be quoted further on.

† "In the splendour of a Neapolitan firmament, we may seek in vain for that *purple light* so delightful to our boyish fancy."—*Tour in Italy.*

‡ Encyclopædia Britannica, art. *Meteorology.*

|| *Bibliothèque Universelle* (1830), tom. xliv. p. 337.—Translated in Taylor's Scientific Memoirs, vol. i.

§ It is a curious circumstance, which I have never heard remarked, that Dr. Priestley in a great measure anticipated the experiment of Nobili; for, by successive electric discharges on the surface of *many* kinds of metal, he produced rings identical with those of Newton.—*Priestley, Phil. Trans.* 1778. These colours were no doubt produced by the heat developed in the same way as those mentioned in one part of Nobili's paper. The explanation of these colours, by supposing with the philosopher of Reggio (if I understand him aright), that they are produced by thin plates of *adhering oxygen gas,* is too evidently founded in error to require any notice.

From an attentive comparison of the beautiful series of tints, identical with those of thin plates, so produced, Nobili endeavours to assign *empirically*, as Newton had done, the orders to which the colours of Nature belong; only, instead of cautiously proposing them as guesses, like his illustrious predecessor, he assigns them, with a degree of confidence but ill sustained by the now almost untenable character of Newton's theory of the colour of bodies. Many of the remarks are very ingenious, but whenever he contradicts Newton, he seems, I think, to fall into evident inaccuracy. The general question is one with which we have now nothing to do, and therefore I confine myself only to the statements which concern the present subject. Because he has banished the *blue of the first order*, as having no existence*, he is forced to assign to the blue of a clear sky the character of the second order; whilst he attributes the tints of flocculent clouds, partially illuminated by the sun or moon, to the first order; in other words, he supposes the vesicular vapour of which he speaks, to have double the thickness in an azure sky, than in the midst of a fog, whilst Newton expressly assigns the blue of the first order to the air, because "it ought to be the colour of the finest and most transparent skies in which vapours are not arrived at that grossness requisite to reflect other colours, as we find it is by experience." This is only one of the various contradictions into which the artist-like view of matching colours by external resemblances, and assuming a common origin, has led the ingenious author. The application of the colours reflected from vapours to measure the thickness of the vesicles† was, we have seen, completely anticipated by Kratzenstein, and the generality of the application disproved by Melvill half a century ago, when he speaks of the theory of the "gaudy colours" of the clouds arising, "like those of the soap bubble, from the particular size of their parts."

I have perused Nobili's Memoir with a most anxious wish

* Nobili quotes Amici's authority in confirmation of this novel assertion, and also for the alleged absence of green in the second order of colours. I think I can speak with much confidence as to the existence of blue of the first order in the depolarized tints of mica plates: but the attempt to show (Bibl. Univ. xliv. p. 343 and 344, *note*), that there *ought* to be no blue, and that the first colour of Newton's scale should be *white*, seems to me a failure, arising from a degree of misconception of first principles which it is difficult to admit.

† In the translation of the paper in Taylor's Scientific Memoirs, i. 99, by an oversight, the maximum thickness of the cloudy vesicles is stated at *the ten-millionth* of an inch, instead of *ten millionths* of an inch, or a hundred times greater, as in the original. There is even a slight mistake in the latter; the tint he describes corresponding to plates of water, not of air, would require a thickness of seven millionths.

to arrive at his true meaning, disembarrassed of the somewhat poetical vagueness of his own expressions, and the serious mistakes of his translator; and I believe his view to be this: —There are both transmitted and reflected tints in the sky. The transmitted ones are complementary to the blue of the sky, and therefore, according to Nobili, of the *second* order, whilst all the fiery tints which particularly characterize sunset as contrasted with the dawn, are colours of the *first* order reflected from the vesicular vapours of clouds.

An ingenious paper by Count Xavier de Maistre on the colour of air and water, appeared in the *Bibliothèque Universelle* for November 1832*. With regard to the atmosphere, the author's theory is so far similar to that of Delaval, that its colour is to be ascribed to the peculiar state of the particles of water contained in it acting on the principle of opalescence, the reflected light being blue and the transmitted orange. He thence refers to the colours of sunset, and adds,—"But it often happens that the colours are not observed, and the sun sets without producing them. It is not, therefore, to the pure air alone that we must attribute the opaline property of the atmosphere, but to the mixture of air and vapour in a particular state, which produces an effect analogous to that of the powder of calcined bones in opaline glass. Neither is it the quantity of water which the air contains that occasions these colours, for when it is very humid, it is more transparent than it is in an opposite state, the distant mountains then appearing more distinct,—a well-known prognostic of rain, and the sun then sets without producing colours; in the fogs and vapours of the morning, the light of the sun is white, but the red colour of the clouds at sunset is generally regarded as the forerunner of a fine day, because these colours are a proof of the dryness of the air, which then contains nothing more than the particular disseminated vapours to which it owes its opaline property." In this interesting passage we have, I am persuaded, all that is known of the cause of atmospheric colours, with the single want of the link which shall show that the watery vapour is sometimes capable of absorbing all but red rays, and sometimes not†.

* Translated in the Edin. New Phil. Journal, vol. xv.

† Count Maistre explains the colour of the water by similar reasoning. He considers it blue for reflected, and yellowish-orange for transmitted light, and the green colour of the sea and some lakes he attributes to diffused particles which reflect a portion of the transmitted tint, and mingle with the blue. This is well confirmed by Davy's Observations, (Salmonia, 3rd edit. p. 317). Arago has very ingeniously applied the same reasoning to the ocean, showing that when calm it must be blue, but when ruffled, the waves acting the part of prisms, refract to the eye some of the transmitted light

The late Mr. Harvey of Plymouth, gives a minute analysis of the colours of the clouds*, which he considers only explicable on the theory of absorption, which office he assigns to the particles of the clouds themselves, though he admits that these often transmit pure white light. He is even ready to believe that the sun has sometimes been observed blue or green, an observation which I think M. Arago has rightly considered as an optical deception arising from the contrasted colour of an intensely red sky, such as that which occurred in many parts of the world on the occasion of the dry fog of 1831†.

Brandes's theory of the evening red is especially applicable to the rich purple hue thrown over Mont Blanc and the higher Alps‡ after the sun has set to the plains, and that kind of redness is usually observed in cloudless skies, not like the gorgeous colouring of our northern sunsets, to which I particularly referred in my former paper. In a communication read to the British Association in 1837, M. De la Rive accounts ingeniously for a repetition of this phænomenon which is sometimes observed 10 or 15 minutes after the first disappeared. This he plausibly attributes to a total reflection undergone by the rays of light in the rarer regions of the atmosphere when in a state of great humidity and transparency ||. Probably upon the principle of multiplied reflections, the cases of preternaturally protracted twilights may be explained, such as those recorded by Kämtz §.

It is now time that we endeavour to sum up briefly the evidence we have collected.

If we exclude the theory of Leonardo da Vinci and Göthe, attributing the colour of the sky to a mixture of light and shade; and that of Muncke, which would make it a mere optical deception, we shall find the chief principles which have been maintained, reduced to three.

(1.) That the colour of the sky is that reflected by pure air, and that all the tints it displays are modifications of the re-

from the interior, and it then appears green (*Comptes Rendus*, 23d July 1838.). Most authors have admitted the intrinsically blue or green colour of pure water, as Newton (Optics, b. i., part ii., prop. x.), Mariotte (already quoted), and Euler: Humboldt seems doubtful (Voyage, 8vo, ii. 133.).

* Encyc. Metropolitana, art. *Meteorology*, p. 163, &c.

† Annuaire 1832, p. 248. Whilst this Paper is passing through the press, I have seen a notice by M. Babinet (*Comptes Rendus*, 25th Feb. 1839), on the subject of the blue colour of the sun, which he considers as real, and endeavours to explain by the theory of mixed plates. [See on this subject the Miscellaneous articles in the present Number.—Edit.]

‡ Germ. "Glühen der Alpen."

|| Seventh Report of British Association. Transactions of Sections, p. 10.

§ *Lehrbuch der Meteorologie*, iii. 58.

flected and transmitted light. This is more or less completely the opinion of Mariotte, Bouguer, Euler, Leslie, and Brandes.

(2.) That the colours of the sky are explicable by floating vapours acting as thin plates do in reflecting and transmitting complementary colours. This was Newton's theory, which has been adopted in whole or in part by many later writers, and especially by Nobili.

(3.) On the principle of opalescence and of specific absorption depending on the nature and unknown constitution of floating particles. To this theory in its various stages, we find Fabri, Melvill, Delaval, Count Maistre, and Sir D. Brewster attached.

These different views are so easily blended, and have often been so far misunderstood even by their supporters, that it is impossible to draw any definite line between them. I will notice a few of the leading points of difficulty which present themselves to some of these opinions, and tend to restrict the field of inquiry.

1. The azure of the sky cannot, I think, with any probability, be referred to the existence of those vesicular vapours which are supposed to act so important a part in the mechanism of clouds. We have no evidence direct or indirect of their existence, whenever the hygrometer is not affected, nor indeed where it does not indicate absolute dampness. The atmosphere we know to be pre-eminently transparent when loaded with uncondensed vapour. That vapour may be colourless, or it may not; the presumption is, I think, that it has no colour, since the blue of heaven is always most fully developed when the dryness of the air is intense; and *that* even at heights which render it in the last degree improbable that any condensed vapour should exist at heights still greater. We are as ignorant of the constitution of the parts of pure vapour, as we are of the parts of pure air: vesicles are *water*, not *vapour*;—to speak of films capable of reflecting definite colours when no *water* exists in the air, or the hygrometer does not indicate absolute dampness, is to speak (as Berkeley said of Fluxions) of the ghosts of departed quantities.

2. Admitting that the blueness of the reflected light of the sky is an inherent quality, of which we can give no account, we must next say that it is running too fast to a solution to admit with Brandes that the red of evening is solely caused by the colour of the air being complementary to its reflected tint. His explanation of the variable redness of sunset, owing to the variable opacity of white vapours allowing the

redness to be more or less distinctly perceived, though ingenious, is palpably wrong. The simplest experiments prove that the redness is not merely apparent, but depends upon the admixture of the variable ingredients of the atmosphere. The proof is the Prismatic Analysis of the sun's light, and we may add, the observation of artificial lights in different states of the atmosphere, which at some times are seen in their natural condition, at others lose all their rays but the red, and finally vanish in fogs with an intense red glare.

3. If fogs and clouds modify the solar light on the principle of reflecting the rays they do not transmit, why do not such fogs and clouds appear vividly blue by reflected light, as Nollet supposed a foggy atmosphere must do to a spectator placed beyond it?

4. If the vesicles constituting the clouds give to the colourless light falling upon them the various hues of sunset, why, in the first place, do we not perceive bows of various hues, as Kratzenstein did in operating on the small scale; and how comes it that clouds, identical in structure, nay, the very same clouds, do not exhibit sunset tints at any other time of day? But the most convincing proof of any, is simply to watch the progress of the solar rays tinging a cloud successively with different hues, just as it would a lock of wool similarly placed; or as it does the snowy Alpine summits. Forster mentions an instance of detached cirrocumuli being of a fine golden-yellow, but in a *single minute* becoming deep red.

5. To these unanswerable difficulties the prismatic analysis of the blue and sunset tints of the sky superadds one conclusive against the theory of Newton as it at present stands. The reflected blue and transmitted red-orange are *not* colours of thin plates. They are derived from all parts of the spectrum by the mysterious process of transmission, which has preserved them and absorbed the rest. It is hopeless at present to inquire what is the mechanical constitution of the medium which has effected this alchemy.

One question, however, which is quite within our reach, remains to be answered. The colours of the sky cannot indeed be explained, if by explanation we mean an ultimate analysis of the mechanism producing them; but the theory of absorption is incomplete until we can show in what part of the course of the rays of light, and under what varying circumstances, the different phænomena of colour may be produced. Hassenfratz observed, that the light of the horizontal sun was deficient, when analysed by the prism, in all the violet and blue rays*. Sir D. Brewster, making a similar observation

* Kämtz, *Lehrbuch,* iii. 40.

with more care, has detected a *specific action* of the earth's atmosphere affecting every part of the spectrum by absorbing or annihilating certain luminous rays of every colour. The analogy which he has observed to exist between the deficient lines of the atmospheric spectrum, and those of the common solar spectrum, (which Sir David supposes to have been produced in the transit of light through the sun's atmosphere), and those developed in artificial light by the absorptive action of nitrous acid gas, is truly remarkable, and has led him further to conclude, "that the same absorptive elements exist" in all those media*. Now, since it is the strata of air nearest to the earth whose effect is chiefly conspicuous in producing the tints of evening, it is to be presumed that the elements which produce this action, are within reach of chemical analysis. The air, containing as it does the constituents of nitrous acid gas, is naturally first looked to for their origin. But this supposition, even if it be true, for the atmospheric *lines* of the spectrum, cannot explain the extraordinary variety of absorptive action observed in hazy weather, when, as we have said, the atmosphere at a thickness of but a few miles suffers only the red rays to pass; a fact familiar to those who have attended to the subject of light-house illumination, and in consequence of which crimson signal-lights were proposed a few years ago for adoption in hazy weather by Sir John Robison†, on account of the persistence of such rays in a foggy atmosphere. The absorptive elements are clearly within our reach; can they be nitrous gas, or what are they? The experiment detailed in my last paper comes in to answer the question. Vapour has hitherto been known (to philosophers at least) under but two characters,—a colourless gaseous body, and a translucent pure white mass of particles generally called vesicular‡. I have shown that it passes through a third or intermediate state, in which it is very transparent, but having a more or less intense colour graduating through the very shades which nitrous acid gas assumes,—that is, tawny yellow, orange, deep orange-red, intense smoke-red, verging on blackness. I say that this discovery, to a great extent, supplies the gap which was wanting to make the absorption theory intelligible. It is the "mixture of air and vapour in a particular state," which Count Maistre supposed (see the passage quoted above), but could not prove to exist. The threefold condition of vapour in the sky we can now exhibit in a room;—the pure elastic fluid devoid of colour, which gives even to

* Ed. Trans. xii. 530. † Phil. Mag. 1833.
‡ See Robison's Works, ii. 2, &c.

pure air its greatest transparency,—next, the transition state, when, still invisible in form, and almost certainly not vesicular, it transmits a steady orange glare, not the play of colour which is often seen in clouds and fogs forming a glory round a radiant body;—and lastly, the vesicular steam, such as we every day see issuing from the spout of a tea-kettle reflecting iridescent colours, just as the semi-opake clouds do which seem to float across the disk of the sun or moon. These coronæ, notwithstanding their apparent analogy to the colours of thin plates, seem rather to be due to the effect of diffraction*.

The non-appearance of the lines of the spectrum in my experiment, may be plausibly explained in the following manner, which, however, I offer merely as a conjecture. When steam of high pressure issues from an orifice, a horizontal section of the expelled column will include vapour in every stage of condensation. Its centre, up to a certain height, will be pure invisible steam; at the exterior of all, in contact with the cold air, there will manifestly be vesicular steam, and a cylindrical space between the two will contain red steam. Now it is extremely probable, that when the experiment is performed on the small scale, as I have described it, by suffering light to pass through such a compound column, and then analysing it by the prism, enough of unabsorbed rays are reflected from the highly luminous surface of the vesicular steam to prevent the fine lines from being seen if they exist. And I am strongly confirmed in this conjecture by the fact, that when the rush of steam is very violent, and always when much vesicular vapour is present, the unabsorbed part of the spectrum presents a washy and impure tint (particularly mentioned in my former paper), which probably arises from a blending of the colours, produced by this cause.

In conclusion, I have only a word or two to say respecting the application of these facts to atmospheric appearances regarded as prognostics of weather. The modified hues of the sky, and of the sun and moon near the horizon, have, for so many ages, and in so many countries, been regarded as the surest indications of atmospheric changes, that we cannot doubt that it is to the variety of conditions in which vapour exists in the air, more or less nearly condensed, that these phænomena are due. Humboldt describes the colour and form of the sun's disc at setting in tropical regions, as the most infallible prognostic†, and elsewhere ascribes these variations "to a particular state of the vesicular vapour‡."

* See Young's article *Chromatics*, in Encyc. Brit., and Fraunhofer in Schumacher's *Astronomische Abhandlungen*. Drittes Heft, 1825.

† Rélation Historique, 8vo, ii. 128. ‡ New Spain (translation), ii. 326.

Since the red steam occurs only during the critical stage of its partial condensation (and perhaps conversely during evaporation), it is evident that it must correspond to a critical state of diffused vapour of the atmosphere. The applications might be very extended; I will only advert to one, the surest, most consistent, and probably the most ancient of such prognostics. The red evening and grey morning as the signs of fine weather, are recorded in the verses of Aratus*, in the New Testament†, and in one of our most familiar proverbs. It is wholly inexplicable on the theory of Brandes, which considers the redness as due solely to the purity of the atmosphere, since that is usually greater in the morning than the evening. According to my view it occurs thus: Soon after the maximum temperature of the day and before sunset, the surface of the ground, and likewise the strata at different heights in the atmosphere, begin to lose heat by radiation. This is the cause of the deposition of dew, and consequently in severe weather we have vast tracts of air containing moisture in that critical state which precedes condensation, and yet it may be exceedingly doubted whether any vapour properly called vesicular is necessarily formed in this process. Be that as it may, every accurate observer of nature in alpine countries will confirm me in stating, that fine weather is almost invariably accompanied by the formation of dew on exposed surfaces, and by the progressive depression of the moister strata, until at length visible fogs are formed in the bottom of the valleys, and especially over water‡. This is the surest sign of a following fine day in mountainous regions. Now Saussure in his ascent of Mont Blanc, "observed that the evening vapour which tempered the sun's brightness, and half concealed the immense space he had below him, formed the finest purple belt, encircling all the western horizon, and as the vapour descended and became more dense, became narrower and of a deeper colour, and *at last of a blood-red*||." Now this phænomenon corresponds, I imagine, precisely to the development of colour which I have remarked in vapour in the act of being condensed, and De la Rive's remark, that the nocturnal illumination of Mont Blanc takes place in serene evenings, *when the*

* Diosemeia, 93. quoted by Kämtz. † Matt. xvi. 2, 3.

‡ For the reason why over water, see Davy's Paper, Phil. Trans. 1819.

|| Quoted by Harvey in Ency. Metrop. *Meteorology*, p. *166. The cause of the purple light mentioned here, probably arises from a mixture of the reflected blue of the pure sky (*which is always present when purple is seen*) with the yellow-orange, which condensing vapour first transmits. I do not think it at all necessary to affirm, however, that pure air has no transmitted colour of its own.

air is highly charged with moisture, is to the same purpose. But a remark of Mr. Forster, in his "Researches about Atmospheric Phænomena*," is even more pointed, and is valuable, because his work is pre-eminently descriptive, rather than theoretical. "Sometimes the tints in the twilight haze come on so suddenly and are so circumscribed, as to induce a belief that very sudden and partial changes take place in the atmosphere at eventide; *which may perhaps be somehow connected with the formation of dew.*" He then records an observation made 2nd November 1822. "Being about four o'clock in the evening, near Croydon in Surrey, I observed a very beautiful western sky, caused by the bright edge and dependent fringes of a light bed of cloud being finely gilded by the setting sun. Some detached cirrocumuli also, which formed the exterior boundaries of the aforesaid cloud, were likewise of a fine golden-yellow, and the same colour appeared in different clouds in other parts of the sky, while the scud-like remains of the nimbus floated along in the west wind below. In the course of about a quarter of an hour, the lofty gilded clouds all assumed a deep red appearance, and the change was effected so suddenly, that while looking at them, I only took my eyes off them for a minute to stop down the tobacco in a pipe that I was smoking, and when I looked up at them again, the colour was totally changed. Now, what renders the phænomenon remarkable is, that it happened just about the period of the vapour point. The descending sun had scarcely had time to make any great difference in the angle of reflection, and it seemed therefore, that some sudden change, produced by the first falling dew, was the cause of this simultaneous change of colour in all the clouds then visible." I confess it seems to me that this passage is nothing short of a demonstration of the truth of my theory of Atmospheric Colour, the more interesting, because I was unacquainted with it until after writing nearly the whole preceding part of this paper.

With regard to the morning the case is very different. In fine weather the strata near the surface of the earth alone, and in the lowest and most sheltered spots, are in a state of absolute dampness. The vapours, which, during the reversion of the process, might probably produce colour, are not elevated until the action of the sun upon the earth's surface has continued long enough to impart a sensible warmth, by which time the moment of sunrise is past, and the sun's disc has risen above the horizontal vapours. It would be easy, by a more lengthened discussion, to show, that the slowly progressive

* Third edit. p. 87.

transition of vast masses of air through the temperature of the dew-point, can only occur in serene weather at sunset and not at sunrise. The inflamed appearance of the morning sky, considered indicative of foul weather, is, I have no doubt, owing to such an excess of humidity being present, that clouds are actually being formed by condensation in the upper regions, contrary to the direct tendency of the rising sun to dissipate them, which must therefore be considered as indicating a speedy precipitation of rain.

Edinburgh, 4th February, 1839.

VII. *Observations on an improved Construction of the Voltaic sustaining Battery.* By F. W. Mullins, *Esq., F.G.S. F.S.S., &c.* *

THREE years have elapsed since I introduced to the public at one of the Friday-Evening Meetings at the Royal Institution, my mode of construction of the voltaic sustaining battery. This battery was then in its simplest form, being nothing more than a coil or cylinder of copper inclosed in a thin bladder containing a solution of sulphate of copper, both being placed in an earthenware pot, holding a cylinder of amalgamated zinc immersed in a solution of muriate of ammonia†. In this arrangement the sulphate of copper solution had communication with the *internal* as well as the external surface of the copper cylinder; but subsequent experiments having convinced me that there was no use, but rather an injury, in permitting access of the sulphate solution to the *internal* surface of the copper, I altered the arrangement so far as to close the bottom of the cylinder, and throw the *whole* of the salt of copper, as it dissolved, upon a shelf at the upper part of the cylinder, through holes in its circumference on a level with the shelf *into the space between the two metallic surfaces*, where of course it was most needed. The first of these arrangements was worked for a short time after it was made public at the Adelaide Gallery of Science, the other subsequently, and both are described in the Phil. Mag. for Oct. 1836. I am thus particular in adverting to the two modes of construction adopted by myself, in consequence of ascertaining that some would-be scientific persons, with less of discretion or honesty than of puerile vanity, have since the period referred to attempted to arrogate to themselves whatever merit there might have been in the arrangement in which the sulphate solution is used internally as well as externally; and I have reason to know that batteries so arranged have been palmed upon many as superior to those which after a long

* Communicated by the Author.

† See Phil. Magazine for October 1836, p. 283.

series of experiments I adopted and recommended in preference. In making these remarks I am quite sure that the readers of the Philosophical Magazine will do me the justice to believe that I am actuated by no other motive than a sincere and honest desire to place in their hands an instrument of research as powerful and as perfect as my investigations enabled me to make it, in place of one which, although as much my arrangement as that subsequently adopted by me, did not certainly afford equally satisfactory results. It is quite clear that the first mode of construction being as much mine as the second, there was no reason why I should have preferred the latter, if I had not clearly ascertained that it was superior to the former; and under such circumstances, the discoveries recently made in the development and applications of metallic electricity being so important, and so certain to lead to extraordinary as well as beneficial results, it becomes my duty, and I will add, the duty of every true lover of science, to endeavour to put into the hands of his fellow-labourers, who may not have equal opportunities with himself of ascertaining their relative merits, those instruments of research which laborious investigation may have proved to him to be best suited to the purposes of further discovery.

With these preparatory remarks, which go to show how my sustaining battery was at first arranged, and subsequently improved by me some three years since, I now proceed to the proper subject of this paper, namely, the description of a further improvement, not only in the mode of construction, but also in what may be properly termed the necessary constituents of the sustaining battery.

Although the introduction of a membranous partition between the zinc and copper surfaces was productive of great advantages, the chief of which was the obtainment of *undiminished* power for a long period, I soon perceived that the battery was still far from perfect, and that the remedy for one defect unfortunately created another. It was true that the deposit of injurious elements upon either metallic surface, or upon both, was prevented by the interposition of membrane; but it so happened, that after a few hours' action pure copper was not only precipitated upon the external surface of the copper cylinder, where *alone* it should have been attracted, but it was also deposited upon the surface of the membrane, and actually formed within its substance, and on the surface next the zinc, thereby creating local action, with its consequence, great waste of zinc, and tending to diminish the general effect. This was a source of great annoyance where it was desirable to keep up the action for a long period; for not only the copper

deposit led to the consequences already described, but by working through the membrane it destroyed its texture, causing it to leak, and thus rendered the battery unfit for use till a new membrane was supplied, which necessarily caused the entire arrangement to be disturbed. Under these circumstances I determined to find a substitute, if possible, for the membrane, which should be free from the defects alluded to, and afford me a more perfect and permanent instrument of research. I will not here enter into a detail of the various substances experimented upon; suffice it to say, that I used different sorts of wood, parchment, different sorts of paper, unglazed earthenware as recommended by Daniell, &c. but without being satisfied with the results. However, in consequence of some remarks made upon the action of the battery when I employed the wooden partitions, I resolved to give them a second trial; but upon this occasion I perceived the expediency of purifying the wood before it was used, and accordingly after the partition had been constructed, I immersed it in boiling water containing a small proportion of sulphuric acid, and kept it therein for about an hour; it was then placed for some time in cold water, and on trying it subsequently in the battery, I found that in the first instance it answered expectation so far as affording a free passage to the electric current; and after a lengthened trial my anticipations were fully realized by the fact, *of all metallic deposit upon the interposed substance being prevented.*

I thus obtained a substance which as a medium of conduction answered all the purposes of membrane or other substances, while it possessed the important advantages of attracting no metallic particles, of non-liability to fracture or other injuries, of being always fit for use*, no matter how long it may have been previously employed, and last, not least, of being infinitely cheaper than membrane or earthenware. I have been using batteries with these partitions for nearly two years, and have every reason to feel satisfied with their action. I will now describe the voltaic arrangements which I at present use, and wish previously to remark, that continued investigation supports my estimation of closed copper cylinders in preference to my original employment of open ones.

In the first form, I use a thin copper cylinder about six inches in height and three in diameter; the bottom and other

* It may be well to state, that previously to employing the wooden cylinders, it is necessary to make them thoroughly moist by placing them for a couple of hours in a vessel of water, with which a very small quantity of sulphuric acid or of common salt has been mixed.

joinings being closed without solder, at least externally; the inside is filled with fine dry sand to the height of $4\frac{1}{2}$ inches, for the purpose of keeping the cylinder steady, and upon the sand is placed a copper shelf soldered to the inner surface of the cylinder; this shelf is meant to hold crystals of sulphate of copper and communicates with the *external* surface of the cylinder by means of a number of holes about $\frac{3}{4}$ths of an inch apart, made in the circumference and on a level with the shelf; by this arrangement there is a constant supply of the salt of copper in solution to the fluid which lodges between the *external* surface of the cylinder and the wooden partition in which it stands; whereas, in the case of the open cylinders, one of their greatest defects was, that the crystals, as they were dissolved, were mixed with the *internal* solution where they were *not* wanted (there being *no zinc surface opposed*) in place of the *external*, where they *were*; and thus, not only was the solution *unequal* in strength, but it was *weakest* in the very part where all the action existed, namely, *between the opposed surfaces* of zinc and copper. The copper cylinder stands in a wooden cylinder made of seasoned sycamore, or other white wood, and wide enough to allow the copper vessel to pass into it with perfect freedom; the bottom of this cylinder is $\frac{1}{4}$th of an inch thick coated over with some non-conducting substance; the cylindrical part not more than $\frac{1}{10}$th, and it can be made without any difficulty by any handy turner. Its height is less than that of the copper by $\frac{1}{4}$th of an inch, and both are surrounded by an open cylinder of zinc (*not less than* $\frac{1}{4}$*th of the height of the copper*) immersed in a solution consisting of equal parts of the saturated solution of muriate of ammonia and of water. The sulphate solution stands sufficiently high in the copper cylinder to admit of its passing through the holes round the shelf; but not higher. The entire arrangement is placed in a glazed earthenware pot, resting on a stand in which two glass cups are fixed for mercury. This is unquestionably one of the simplest and cheapest forms of battery ever constructed. Of its power or utility I will only say that it satisfies me, and that if those who want such instruments will *only judge for themselves*, I am quite confident of its satisfying any impartial inquirer.

For intensity effects I use a compound battery of ten, in which each cylinder is only $3\frac{1}{2}$ inches high and 2 in diameter, the other parts being in proportion and the mode of arrangement being similar*: and for increased quantity effects

* Both batteries may be seen in action at the Adelaide Gallery of Science, as well as those upon my original plan.

I employ two copper cylinders of the same height as that in the larger battery, and two wooden ones standing *between* the copper cylinders, a cylinder of zinc being placed *between* the former, by which means both surfaces of the zinc are opposed to surfaces of copper, and greatly increased power is obtained in the same space. This mode of arrangement, however, being somewhat more complicated and expensive, may not be approved of so much for general use, nor be employed except in cases where very powerful quantity effects are required.

In conclusion, I would add that $\frac{1}{4}$th of the quantity of the solutions used in other batteries of the same extent of surface will suffice for those described, without any loss of power.

In the next Number of the Philosophical Magazine, I hope to be able to make some observations upon the proper proportions of zinc and copper in voltaic combinations, and in reference to the investigations of Mr. Binks upon the same subject, which have much interested me.

Cheltenham, June 2, 1839.

VIII. *A Geometrical Proposition. By* James Thomson, *LL.D., Professor of Mathematics in the University of Glasgow.*

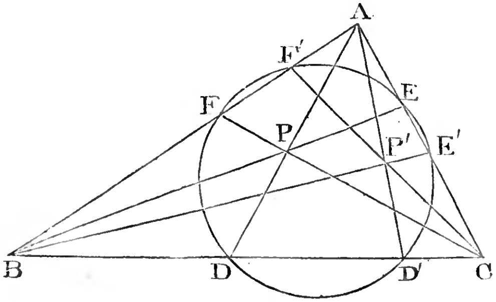
To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I SHALL feel obliged by your inserting in your Magazine the following proposition, which I believe to be new, and which is certainly curious.

Glasgow College, June 10, 1839. James Thomson.

Let A B C be a plane triangle, and through any point P in its plane, let A D, B E, C F, be drawn, cutting the sides,



or the sides produced, in D, E, F: through D, E, F describe a circle cutting the sides, or the sides produced, in D′, E′, F′: join A D′, B E′, C F′: these lines all pass through a common point P′.

For, by the theory of transversals, (Carnot, *Essai sur la Théorie des Transversales*, *Théor*. V.),

$$A F . B D . C E = F B . D C . E A.$$

But (Euc. III. 35. or 36.)

$$A F = \frac{A E . A E'}{A F'},\ B D = \frac{B F . B F'}{B D'},\ \text{and}\ C E = \frac{C D . C D'}{C E'}.$$

Substitute these in the first member of the preceding equation, and free the result of fractions: then, dividing by the quantities common to both members, we get

$$A E' . B F' . C D' = C E' . A F' . B D'.$$

Hence, by the theory of transversals, (Carnot, *Essai sur la Théorie des Transversales*, *Théor*. V.),
$A D'$, $B E'$, $C F'$ all pass through the same point.

Cor. Hence, by supposing D and D′, E and E′, F and F′ to coincide, it follows, that the three straight lines drawn from the angles to the points in which the sides touch the inscribed circle, pass through the same point; and the same holds regarding the points of contact of any of the circles touching a side and the continuations of the other two.

Several curious questions and considerations, arising from this theorem, will naturally suggest themselves, particularly some regarding loci. I shall not, however, enter on such inquiries at present.

IX. *On Pyroxylic Spirit and its Compounds. By* Thomas Thomson, *M.D., F.R.S., &c. and Regius Professor of Chemistry in the University of Glasgow.**

DUMAS informs us that this remarkable substance was discovered by Mr. Philip Taylor in 1812, but that he did not make it known to the public till 1822, in a letter to the Editors of the Philosophical Journal †. Never having seen this communication of Mr. Philip Taylor, I do not know the evidence‡ which he has brought forward in proof of his

* From his "Chemistry of Organic Bodies—Vegetables," p. 346.

† *Chimie appliquée aux Arts*, v. 422.

‡ [See L. & E. Phil. Mag. vol. vii. p. 395, 427; vol. viii. p. 85. If Dr. Thomson will refer to vol. lx. p. 315. of the Phil. Mag. and Journal (for Oct. 1822), quoted by M. Dumas merely as the Philosophical Journal, he will find Mr. Philip Taylor's statement that this spirit was known to him in 1812; and some years before he published this account of it in 1822, I had received a portion of it from him. I feel satisfied that Dr. Thomson must be in error as to dates when he mentions that he was in the habit of using it in 1816; I am certain that it was not employed in London at that period, and what proves this almost to demonstration, is the following addition to Mr. Philip Taylor's announcement of his discovery: "I have sent Mr. Garden, of 372, Oxford-street, some of the pyrolig-

being the discoverer. But I have been myself in the habit of using it in lamps ever since 1816. And when I went to Glasgow in 1817, I found that it was prepared, for sale in that city, by Messrs. Turnbull and Ramsay. It was, undoubtedly therefore, well known in this country long before 1822, the date of Mr. Taylor's communication.

When wood is distilled for the purpose of obtaining acetic acid the pyroxylic spirit is formed and found in the aqueous liquid which comes over. It is decanted off to separate it from the tar which comes over at the same time. This aqueous liquid being subjected again to distillation, it is in the first tenth part of the product that we are to look for the pyroxylic spirit. By repeated rectifications, it is obtained in a state of considerable purity. The last rectifications must be made over quick lime, partly to remove water and partly some other impurities. The quantity of ammonia disengaged when the lime is added, is considerable. This ammonia was in combination with acetic acid.

Pyroxylic spirit is pure when it does not become coloured by exposure to the air and light, when it mixes with water in all proportions without becoming muddy, and when it does not form a black precipitate when mixed with protonitrate of mercury, and has no action on paper stained with vegetable colours. The quantity of it contained in the liquid obtained by distilling wood is about 1 per cent.

Mr. Kane, of Dublin, published an interesting set of experiments on pyroxylic spirit in 1836 *. He informs us that he had begun the examination of it before the appearance of Dumas's paper; and I remember hearing a paper on the subject, read by him, at the meeting of the British Scientific Association at Dublin, in 1835. He purified the spirit by saturating it with dry chloride of calcium. The saturated solution crystallizes in long shining six-sided tables. He distilled these crystals over the water-bath, as long as they yielded spirit, and then adding water, continued the distillation.

It is colourless, very fluid, and has a peculiar smell, at once alcoholic and aromatic, and mixed with the odour of acetic æther.

neous æther (pyroxylic spirit) that any gentleman wishing to examine it may know where to obtain it." Now if it had been used in lamps in 1816 it would have been perfectly ridiculous to have pointed out where it might be obtained, rather as a curiosity than for consumption, in 1822.—R. P.

The date of the discovery has been put out of all doubt by an entry of December 10, 1812, in the Process Book of the manufactory in which Mr. Philip Taylor was then engaged, an extract from which has been sent us by Mr. John Taylor, the Treasurer of the Geological Society.—R. T.]

* *Ann. der Pharmacie,* xix. 164. [L. & E. Phil. Mag., vol. vii. p. 397; x. 45.—Edit.

It boils by my trials at 150°. Dumas and Peligot state the boiling point to be 151°·7 *, under a pressure of 29·96 inches of mercury; Macair and Marcet, 150° †; while Leopold Gmelin makes it as low as 137° ‡; and Kane found it 140° §.

The tension of its vapour at 5·70° is 3·27 inches of mercury. The specific gravity of the liquid at 68° is 0·798. So that in this respect it does not differ much from alcohol. The specific gravity of its vapour at the temperature of boiling water is 1·120, that of air being unity.

It was analyzed by MM. Dumas and Peligot, and by Mr. Kane, of Dublin, who obtained

	D. & P.	Kane.			
Carbon	37·22	37·15	1 atom =	0·75 or per cent.	37·5
Hydrogen	12·46	12·39	2 atoms =	0·25 — —	12·5
Oxygen	50·32	50·46	1 atom =	1·00 — —	50·0
	100·00	100‖		2·00	100

We might consider it as a compound of 1 atom carbohydrogen, and 1 atom water. But the analogy of æther renders it more likely that it is an oxide of dicarburetted hydrogen.

Dumas and Peligot found the specific gravity of the vapour of pyroxylic spirit to be 1·120, while Kane obtained 1·121, or almost exactly the same result.

Now, 1 volume carbon vapour weighs	$0·416\dot{6}$
2 volumes hydrogen gas weigh	$0·138\dot{8}$
½ volume oxygen gas weighs	$0·555\dot{5}$
	1·1111

Hence, it is obvious that the vapour consists of 1 volume carbon, 2 volumes hydrogen, and half a volume oxygen, condensed into 1 volume.

* *Ann. de Chim. et de Phys.*, lviii. 10.

† *Bibliothèque Universelle*, xxiv. 126.

‡ *Handbuch der Theoret. Chimie*, ii. 344.

§ *Ann. der Pharmacie*, xix. 165.

‖ Liebig obtained

Carbon	54·20
Hydrogen	11·11
Oxygen	34·69
	100

But the specific gravity was 0·804. The substance analyzed was different from pyroxylic spirit. Liebig got it from L. Gmelin.—*Ann. der Pharm.* v. 32.

Pyroxylic spirit may be preserved without alteration in a vessel, though imperfectly corked. But when its vapour mixed with air is left in contact with spongy platinum, much heat is evolved, and formic acid is formed.

To make this experiment with ease, let a glass cylindrical jar, open at both ends, be placed upon a large plate containing distilled water. Put 3 or 4 capsules, containing from 200 to 300 grains of spongy platinum within the jar, and also some pyroxylic spirit in a wine glass, and within the glass jar. By degrees the vapour of the pyroxylic spirit diffuses itself through the glass jar, and the reaction commences whenever a mixture of this vapour and air comes in contact with the spongy platinum. Abundance of vapours condense on the sides of the glass, which trickle down into the water, and give it an acid taste. If the pyroxylic spirit be renewed in proportion as it evaporates, the liquid in a few days contains enough of acid to enable us to ascertain that it is impregnated with *formic acid*. When alcohol is treated in the same way, *acetic acid* is formed.

What happens in this case will be understood by inspecting the following formulæ:

2 atoms of pyroxylic spirit are $C^2\ H^4\ O^2$
1 atom of formic acid .... $C^2\ H\ O^3$

Hence, to convert 2 atoms of pyroxylic spirit into 1 atom of formic acid, we must abstract 3 atoms of hydrogen, and add 1 atom of oxygen. The oxygen of the atmosphere, by the intervention of the spongy platinum, converts 3 atoms of hydrogen into water, and adds 1 atom of oxygen.

If we let the pyroxylic spirit fall, drop by drop, on the spongy platinum, it becomes incandescent, and the spirit burns, producing carbonic acid in great quantity.

Chlorine acts upon pyroxylic spirit much less violently than upon alcohol. When it is poured into a phial of dry chlorine gas, hardly any heat is evolved, and the action is slow, even when assisted by the solar influence. Even when chlorine and pyroxylic spirit are agitated together, the action is very slow. It it necessary to distil the liquid a number of times in contact with chlorine. Two liquids a reproduced very different in their volatility. The least volatile combines with ammonia, and forms a crystallizable compound.

But Mr. Kane found, that when dry chlorine gas and vapour of pyroxylic spirit come in contact, an explosion takes place. He passed a current of chlorine through the spirit to saturation. Much muriatic acid was formed. He obtained two liquids. The lightest was very acid. The other was thick, had nearly the specific gravity of sulphuric

acid, had a sharp and biting taste, and reddened litmus, doubtless because not quite free from muriatic acid. It was analyzed by Mr. Kane, who obtained

Carbon	21·52
Hydrogen	1·56
Oxygen	10·35
Chlorine	66·57
	100·00

From these numbers we might deduce the following formula:

$2\frac{3}{4}$ atoms carbon	= 2·0625	or per cent.	20·75
1 atom hydrogen	= 0·1250	— —	1·25
1 atom oxygen	= 1·0000	— —	10·10
$1\frac{1}{2}$ atom chlorine	= 6·7500	— —	67·90
	9·9375		100

But this formula is so unlikely to represent the true constitution of a body, that it would be wrong to adopt it without further investigation.

When pyroxylic spirit is distilled with a solution of chlorite of lime (or bleaching powder) a liquid is obtained, to which Dumas, who investigated its nature, has given the name *chloroform.* It has been already described in a preceding chapter.

It was discovered about the same time by MM. Soubeiran* and Liebig†, by distilling a mixture of alcohol and solution of chlorite of lime. But Dumas assures us that its nature is the same, whether we employ alcohol or pyroxylic spirit‡.

Pyroxylic spirit dissolves potash and soda. The solutions are similar to those of alcohol. They become brown-coloured when exposed to the action of the atmosphere.

When pyroxylic spirit, concentrated as much as possible, is brought in contact with barytes, it becomes hot, dissolves the base, and remains combined with it. To obtain the solution pure, we must add barytes in powder to absolute pyroxylic spirit, filter the solution, and evaporate *in vacuo.* A crystalline compound remains, composed of

Barytes	70·5	or 1 atom	= 9·5
Pyroxylic spirit	29·5	or 2 atoms	= 4
	100		13·5

When this compound is cautiously distilled, it furnishes a liquid similar to pyroxylic spirit; then melts and yields an

* *Ann. de Chem. et de Phys.*, xlviii. 131.
† *Ibid.* xlix. 146. ‡ *Ibid.* lviii. 15.

oily product. Finally, it blackens slightly, and the barytes is left in the state of a carbonate.

When hot pyroxylic spirit is saturated with barytes, it deposits on cooling, silky needles, which speedily become brown when exposed to the air.

Pyroxylic spirit dissolves salts almost like alcohol. It precipitates the sulphates from aqueous solutions. It dissolves chloride of calcium in abundance, and forms with it a crystallizable compound.

It dissolves the resins, and as it is more volatile than alcohol, it answers exceedingly well for making varnishes. It is not so good a solvent of very hydrogenous bodies as alcohol; but it is an excellent solvent of bodies which contain much oxygen.

When a mixture of 1 part of pyroxylic spirit, and 4 parts of concentrated sulphuric acid is distilled, a gas comes over, which possesses exactly the constitution of alcohol vapour.

It has an æthereal smell, is totally soluble in water, and burns with a flame similar to that of alcohol. At first it is mixed with carbonic acid gas, and sulphurous acid gas; but if it be left for 24 hours in contact with fragments of potash, these impurities are dissolved. The specific gravity of this gas, as determined by MM. Dumas and Peligot, is 1·617.

A volume of it requires for complete combustion 3 volumes of oxygen gas, and forms 2 volumes of carbonic acid. Hence it contains

$$\left.\begin{array}{ll} \text{2 volumes carbon......} & 0{\cdot}833\dot{3} \\ \text{2 volumes hydrogen} & 0{\cdot}138\dot{8} \end{array}\right\} \begin{array}{l}\text{condensed into} \\ \quad\text{1 volume.}\end{array}$$

$$\overline{\quad 0{\cdot}9722 \quad}$$

If we subtract 0·9722 from 1·617, the specific gravity of the gas, the remainder 0·6248 is almost exactly equal to the specific gravity of a volume of vapour of water, namely, 0·625. Hence it is obvious that this vapour is a compound of

$$\begin{array}{ll} \text{1 volume olefiant gas...} & 0{\cdot}972\dot{2} \\ \text{1 volume vapour of water} & 0{\cdot}625 \\ \hline & 1{\cdot}5972 \end{array}$$

condensed into 1 volume; which gives precisely the elements that enter into the constitution of alcohol.

The very same thing takes place in this distillation as when we heat a mixture of alcohol and sulphuric acid. One half the water is abstracted relative to the other ingredient, the carbohydrogen. When alcohol is used, the deutocarbohydrogen, or olefiant gas, is converted into æther; but when

pyroxylic spirit is used, the compound is $C^2 H^3 O$, or it contains an atom of olefiant gas less than æther. This is the same thing in both cases as abstracting one half of the water which the spirit contained. But in reality

Alcohol is .... $C^4 H^5 O + H O$
While this gas is $C^2 H^2 \quad + H O$

We see the reason why its properties are so different from those of alcohol.

Action of the hydracids on pyroxylic spirit.—When pyroxylic spirit is made to act on the hydracids, a set of compounds is formed very analogous to the æthers which the same acids form with alcohol. These bodies have been examined by Dumas and Peligot, who consider them as compounds of the hydracid employed and methylene, which in their opinion acts the part of a base*.

1. *Chloride or muriate of methylene.*—This compound,

* Dumas and Peligot have given the *methylene* to what they consider to be the base of pyroxylic spirit, and which they make $C^2 H^2$, and pyroxylic spirit they make $C^2 H^2 + H O$. But the subject will be much simplified if we apply Liebig's theory of æthers to pyroxylic spirit, with the requisite modification. The base of pyroxylic spirit will be $C^2 H^3$, and pyroxylic spirit will be $C^2 H^3 O$. This base has not yet been insulated, but the following salts are obviously the chloride and iodide of $C^2 H^3$, which we may, after Dumas and Peligot, denominate methylene.

It follows from the experiments of Dumas and Peligot, that pyroxylic spirit is $C H^2 O$. Perhaps it would be better to double these numbers, and to consider it as $C^2 H^3 O + H O$. It would then bear the same relation to the base which these chemists have distinguished by the name of methylene, that alcohol does to æther. On that view we might consider the unknown basis of pyroxylic spirit to be $C^2 H^3$, or *methyl.* Of this pyroxylic spirit is the hydrated oxide. The other compounds, by Dumas and Peligot's analysis, are

Chloride or muriate of methylene..	$C^2 H^4 + Chl^2$
Iodide or hydriodate.............	$C^2 H^3 + Iod$
Fluoride or fluate	$C^2 H^3 + Fl$

The salts of methylene, analogous to the acid æthers, are the following:

Sulphate	$C^2 H^3 O + S O^3$
Nitrate	$C^2 H^3 O + Az O^3$
Oxalate..................	$C^2 H^3 O + C^2 O^3$
Acetate..................	$C^2 H^3 O + C^4 H^3 O^3$
Formate	$C^2 H^3 O + C^2 H O^3$
Benzoate	$C^2 H^3 O + C^{14} H^5 O^3$
Mucate..................	$C^2 H^3 O + C^6 H^4 O^7$
Oxychlorocarbonate	$C^2 H^3 O + C^2 Chl O^3$
Chlorocyanate	$C^2 H^3 O + (C^2 Az) Chl$
Cyanate	$C^2 H^3 O + 2 (C^2 AzO) + 3 (H O)$

The acidulous methylene salts, similar to althionic acid, and the other compound æthereal salts, are the following:—

Sulphomethylic acid......	$C^2 H^3 O + 2 (S O^3)$
Tartromethylic	$C^2 H^3 O + 2 (C^4 H^2 O^5)$
Racemomethylic	$C^2 H^3 O + 2 (C^4 H^2 O^5)$

analogous to muriatic æther, is most conveniently obtained by heating a mixture of two parts of common salt, one part of pyroxylic spirit, and three parts of concentrated sulphuric acid. By the application of a gentle heat, a gas is obtained, which may be collected over water, and which is pure muriate of methylene.

This gas retains its elasticity though cooled down to zero, or even a degree lower. It is colourless, has an æthereal odour, and a sweet taste. It burns with a flame, white in the middle, and green round the edges. Water dissolves 2·8 times its bulk of it, at the temperature of 61°, and when the barometer stands at 30 inches. It does not alter vegetable blues, nor does it precipitate nitrate of silver. When detonated with an excess of oxygen gas, it is decomposed, and the products are water, carbonic acid, muriatic acid, and some traces of chlorine. The water formed is sufficient to condense the muriatic acid disengaged. Each volume of gas requires $1\frac{1}{2}$ volume of oxygen, and produces a volume of carbonic acid gas. Hence it is obvious that every volume of the gas contains a volume of carbon vapour, and a volume of hydrogen gas, united into a volume of carbohydrogen. The specific gravity of gaseous chloride of methylene is 1·7310

That of carbohydrogen 4861

1·2449

Now, the specific gravity of muriatic acid gas is $1{\cdot}2847\dot{2}$, almost identical with this residue. Hence there can be no doubt that the chloride of methylene is composed of C H^2 Chl. It is therefore pyroxylic spirit, with an atom of chloride substituted for an atom of oxygen.

When this gas is made to pass through a red-hot porcelain tube, it undergoes complete decomposition, being converted into muriatic acid and a carburetted gas, while the porcelain tube is lined with a thin coat of charcoal. The carburetted gas is simple carbohydrogen, for a volume requires for complete combustion 1·5 volume oxygen gas, and forms 1 volume of carbonic acid.

2. *Iodide or hydriodate of methylene.*—This compound is easily obtained by distilling a mixture of 1 part of phosphorus, 8 parts of iodine, and 15 parts of pyroxylic spirit. The iodine is dissolved in the pyroxylic spirit. The solution is put into a retort, and the phosphorus added by little and little. The first fragments added occasion a lively effervescence, with the evolution of heat, and the production of hydriodic acid. When the ebullition thus produced is at an

end, the rest of the phosphorus is added, and the whole agitated. By and by heat must be applied, otherwise the effervescence would cease altogether. The distillation is continued as long as an æthereal liquid continues to pass.

The residue in the retort contains phosphorous acid, phosphomethylic acid, and phosphorus. It is quite deprived of colour. The liquid in the receiver is composed of pyroxylic acid and iodide of methylene. When water is added to the mixture the iodide immediately separates. The quantity obtained nearly equals the weight of the iodine employed. It is still very impure. To obtain it in a state of purity, we must distil it over the water-bath with chloride of calcium, and litharge in great excess.

It is a colourless liquid, weakly combustible, burning only when put into the flame of a lamp, and then giving out violet vapours in great abundance. Its specific gravity at 71° is 2·237. It boils when heated to between 100° and 120°.

Dumas and Peligot analyzed it by means of oxide of copper, and obtained

Carbon	8·92 or 2 atoms	= 1·5	or per cent.	8·51
Hydrogen	2·23 or 3 atoms	= 0·375	— —	2·12
Iodine	88·85 or 1 atom	= 15·75	— —	89·37
	100·00	17·625		100

or C^2 H^3 Iod.

Dumas and Peligot found the specific gravity of the vapour of iodide of methylene 4·883. This specific gravity would indicate

1 volume carbon	0·4166
2 volumes hydrogen	0·1388
1 volume iodine vapour	8·8000
	2)9·3555
	4·6777

condensed into 2 volumes. It is obvious that there is an error in determining the composition of the iodide, or in that of the specific gravity of the vapour.

3. *Fluoride or fluate of methylene.*—This compound was obtained by Dumas and Peligot* in the following manner:

A mixture of fluet of potassium and sulphate of methylene was gently heated in a glass vessel. Sulphate of potash was formed, and a gas escaped, which being collected over water, was pure, and constituted fluate of methylene.

It is colourless, has an æthereal smell, and burns with a flame, similar to that of alcohol, only a little more mixed

* *Ann. de Chim. et de Phys.* lxi. 193.

with blue. During its combustion, fluoric acid is developed, and appears in white fumes.

It is but little soluble in water, 100 of water at 60°, absorbing 166 volumes of the gas. Its specific gravity was 1·186. It was composed of

1 volume methylene
1 volume fluoric acid

united together, and condensed into 1 volume; or it is composed of

1 atom fluoric acid	......	1·25
1 atom methylene	......	1·875
		3·125

So that its atomic weight is 3·125.

4. *Action of the oxacids on pyroxylic spirit.*—When the oxacids are made to act upon pyroxylic spirit, two different compounds are formed; one corresponding with the æthers formed by means of the same acids and alcohol, and which are in reality neutral salts; another constituting acid salts, and corresponding with althionic and other similar acids.

The former, which are perfectly neutral, are obtained more easily than the corresponding alcohol æthers. They all contain an atom of $C^2 H^3 O$, united to an atom of the acid. They are more volatile, and more stable than the corresponding alcoholic compounds.

[To be continued.]

X. *Analysis of Sea-water as it exists in the English Channel near Brighton.* By G. Schweitzer*, M.D.

BEING unaware of the existence of a correct analysis of sea-water as it exists in the British Channel, particularly with reference to the quantity of iodine and bromine it contains, I have undertaken at the request of several friends to analyse it. It is not my intention to enter into the minutiæ of the process employed, particularly as I have on a former occasion, in a small pamphlet entitled "An Analysis of the Congress Spring of Saratoga in America," published in March 1838, given a detailed account of the mode I adopt in analysing mineral waters. The chief object I have in view in the present communication is, to explain the method I have employed in ascertaining the proportion of iodine and bromine contained in a given quantity of sea-water. But before I enter upon the subject, it may not be out of place to show how far tests act upon iodine when in connexion with an

* Communicated by the Author.

alkali, and in a solution also containing bromides and chlorides.

From experiment I have ascertained that a minute quantity of iodine in distilled water, equal to no more than 1,500,000th part of the whole, will be distinctly indicated when mixed with starch, dilute sulphuric acid, and chlorine.

For the production of such delicate reaction, I add to every 500 grains of fluid one drop of diluted sulphuric acid, a small quantity of paste of potato starch, and two drops of a weak solution of chlorine, consisting of one part of a saturated solution diluted with 20 to 25 times its volume of distilled water. The solution gives no indication of the presence of iodine in the fluid until a sufficient time has been allowed for the separation of the starch, when a decided pink hue will be visible on the surface of the precipitate if iodine be present. It has been supposed that the substitution of pink for blue in the iodide of starch produced arises from the presence of bromine; but this I have ascertained is not correct, as it depends entirely on the minute quantity of the precipitate acted upon by free chlorine or bromine. The following experiment will prove this fact. In order to ascertain the delicacy of electrolytic tests of iodine, a current of electricity produced by voltaic induction was passed through a suitable glass tube, filled with 300 grains of distilled water containing $\frac{1}{500,000}$th part of its weight of iodide of potassium and a small quantity of starch, but no action was observed until a few drops of nitric acid were added, which assisting the electric current, developed, after a few brisk revolutions of the coils of the magnet, the blue colour of the iodide of starch. Even a current of electricity from a single constant galvanic battery passed through the same glass tube, in which the proportion of iodide of potassium was only one millionth part of the weight of the water, indicated the presence of iodine by a pure blue speck of iodide of starch at the anode or negative extremity of the electric circuit. When iodide of potassium diluted in the same manner was properly treated with starch, sulphuric acid, and chlorine, the blue iodide of starch likewise became visible, but the smallest additional proportion of chlorine occasioned a pinkish sediment. The presence of chlorides and bromides, however, do not interfere with the action of the electric current upon traces of iodine; for a solution of salts containing, in 500 grains of water, 100 grs. of chloride of sodium, 10 grs. of bromide of sodium, and the five hundred thousandth part of iodide of potassium gave a deposit of iodide of starch of a dark pinkish colour. A concentrated solution of bromide of sodium, containing the millionth part of iodide of potassium,

also gave by the action of the electric current a slightly pinkish deposit.

It is always necessary, when we wish to detect by means of chlorine minute quantities of an iodide, to employ the chlorine in a very diluted state, as when in excess it forms a soluble chloride of iodine which will not act on starch.

The sulphates and chlorides present in salt waters do not interfere with the delicacy of the starch test; on the contrary a concentrated solution of the chlorides will show the presence of one millionth part of iodide of potassium more distinctly than an equal volume of distilled water. This appears to arise from the iodide being a little soluble in pure water. I thought at first that a trace of an iodide might be contained in the common chloride of sodium, and thus cause a deeper tinge of blue colour; but by employing a chloride of sodium prepared from pure hydrochloric acid and pure soda, I found the same degree of increased reaction. The iodide of starch will likewise keep unchanged much longer in a solution of chlorides exposed to light and air than in pure water.

The bromides when present in large quantity interfere with the delicate reaction upon traces of iodine, but when the quantity of iodine is not too small the reaction is very distinct, as a small proportion of free bromine will, like chlorine, decompose the iodide, and produce the characteristic reaction.

After these experiments I tested fresh sea-water for iodine in the manner before described, but did not obtain the slightest indication of it. I now added one millionth part of the iodide of potassium, and the colour produced by the test did not differ in the slightest degree from a solution of chlorides of the same specific gravity as sea-water, treated in the same manner, and from this I immediately inferred, that iodine, if present in sea-water, must be so in very minute quantity.

I took 73 pounds troy, of sea-water and boiled with a quantity of caustic potash, sufficient to precipitate the alkaline earth, and after filtration evaporated the fluid to four ounces. On testing a small quantity of this concentrated water no iodine was to be detected, and it was found on adding a minute quantity of an iodide that the presence of bromides in comparatively large quantity interfered with the test. But although these results appeared to negative the presence of iodine, I felt convinced it must exist in sea-water, being present in so many sea plants and animals.

Sarphate, in his "*Commentatio de Iodio*," 1835, *Leiden* (a treatise which received the prize), states that he could detect no iodine in the sea-water near the Dutch coast. Professor

Charles Daubeney likewise mentions, in his "Memoir on the occurrence of iodine and bromine in certain mineral waters of South Britain, May 1838," that he could not detect iodine in the residuum of sea-water taken from the English Channel near Cowes, after having reduced ten gallons to less than half an ounce.

To proceed with my experiment, I freed three ounces as much as possible from the chlorides by crystallization, having first carefully neutralized the solution with hydrochloric acid. The residuum was then evaporated to dryness, ignited, and treated with anhydrous alcohol. The alcoholic fluid was afterwards evaporated, and the dry residue dissolved in a few drams of water, when the before-mentioned test readily indicated a slight trace of iodine.

With respect to the quantity of iodine in sea-water, it is evidently very minute, 174 pounds troy not containing one grain. This is remarkable when we consider the comparatively large quantity of iodine and bromine present in sea plants and animals, hence we must conclude that these principles are concentrated by vital action.

Bromine when present in fluids is easily detected by chlorine, which produces a yellow colour. If present in very minute quantity the fluid must first be concentrated. But when iodine is present we cannot apply this test, as bromides and iodides are both decomposed by it; and we cannot separate them, even by means of æther, as iodine is soluble in that menstruum, and also possesses greater colouring properties than bromine. From these causes this test is useless when iodine is present, and is only certain when we are previously assured of the absence of that substance.

The following process for the separation of iodine, chlorine, and bromine in fluids containing these substances in very small quantities has given me satisfactory results, as I had anticipated by previous experiment. The fluid while boiling was mixed with a sufficient proportion of caustic potash; my object in this was to decompose the earthy salts, and at the same time prevent the iodine and bromine from being dissipated by heat. The filtered fluid was then evaporated to dryness and ignited, and the resulting mass, after having been dissolved, concentrated, and neutralized with hydrochloric acid, was carefully mixed drop by drop with an ammoniacal solution of chloride of silver prepared by mixing one part of a saturated solution of recently precipitated chloride of silver in ammonia with one of liquid ammonia (sp. grav. 0·935) and two parts of water. If to a concentrated solution of chloride of sodium containing one thirtieth part of a bromide, we add

a few drops of this ammoniacal solution of chloride of silver, the solution will remain clear; but if the most minute particle of an iodide be present, it will be rendered turbid.

To the fluid under examination I added gradually, drop by drop, the solution of ammonia chloride of silver, leaving time between each successive addition for the precipitate of iodide of silver to subside. It is well when bromides are present to keep the vessel closed during the process, otherwise it is of no importance. The iodide of silver collected upon a small filter was first washed with a little diluted ammonia, and afterwards with a few drops of diluted hydrochloric acid to dissolve any earthy substance which the precipitate might contain, and ultimately with pure water.

The filter with the precipitate was dried and ignited. This experiment, repeatedly performed, yielded the most satisfactory results. It requires time, but this is more than balanced by its accuracy. Thus, for instance, I obtained by the analysis of the Congress spring of Saratoga, from 100,000 grs. of the water, 0·12164 gr. of iodide of silver, representing in 1000 grs. of the mineral water, 0·00067 gr. of iodine.

The ammoniacal fluid, separated from the iodide of silver, was carefully evaporated to expel the ammonia, whereby a small precipitate was obtained, consisting of bromide of silver, which was added to that subsequently obtained. This precipitate was formed by the solution of the chloride of silver, more of which was added than was required for the separation of the iodine. That this minute precipitate consisted of bromide of silver, was proved by heating it in a test tube with concentrated sulphuric acid, whereby it became of a delicate yellow colour; whereas chloride of silver would have remained white, and iodide of silver would have obtained a brown colour by parting with its iodine.

A small portion of the fluid may now be examined for bromine, and, when present, the following process may be adopted, which is the same I employed for the separation of bromine in sea-water and brine-springs, where the quantity of chlorides is comparatively very large. The concentrated solution freed from the iodine was introduced into a glass ball, having at its lower end a glass tube, and at its upper an aperture closed by a glass stopper. A concentrated aqueous solution of chlorine was added as long as any sensible yellowness was caused by its addition. The fluid was then agitated with pure æther; and after this had collected on the surface, carrying with it the bromine and chlorine, the water was allowed to flow off through the tube below, and by careful manipulation the æther could then be freed from the water, which

was again treated with æther, lest any bromine should still remain in it. The æther was directly introduced into a glass bottle, containing a solution of caustic potash fully sufficient to discolour the æther, when after evaporation and ignition it was dissolved in water, and carefully neutralised with hydrochloric acid. The concentrated solution was mixed with a few drops of an ammoniacal solution of chloride of silver prepared thus: one part of a concentrated solution of chloride of silver in ammonia, mixed with one part of ammonia and one part of water. A few drops of this mixture produced no turbidness in a solution of chloride of sodium, but indicated a very minute quantity of bromine. When no further turbidness was produced by an additional drop of this ammoniacal solution of the chloride of silver, the fluid under treatment, which was kept in an open vessel, was heated in a sand-bath until the ammonia was almost evaporated. A few drops of the test were again added, until it no longer produced turbidness, when the glass vessel was again placed in a sand-bath, until the fluid, after having been heated, gave no further indication of bromine; it was then tested again with chlorine. When the proportion of the chlorides to the bromides is not too large, scarcely a faint yellowness will be produced; if, however, it is, the bromine must again be separated by chlorine and æther, and the before-mentioned process repeated, when the last traces of bromine will be separated as bromide of silver, which is to be treated like the iodide of silver before it is weighed. In this manner I have been able to detect the smallest proportion of an iodide and bromide when accompanied by a great quantity of chlorides, and have also been enabled to separate them and to ascertain their respective quantities. Should the quantity of iodine be much larger than that of bromine, it would be requisite to evaporate a little of the ammonia; and although the addition of the ammoniacal solution of chloride of silver, employed as a test for iodine, no longer produces turbidness, it is still necessary to add another drop of the precipitating fluid, in order to ensure the separation of every trace of iodine. This is the more important, as the iodide of silver is not entirely insoluble in ammonia; and although the quantity dissolved might be exceedingly minute, still this repetition is necessary in an accurate analysis. The same precaution must be observed in the separation of bromine, as bromide of silver is to some extent soluble in ammonia, for it is obvious that by the addition of the ammoniacal precipitant for every portion of bromide of sodium or potassium, an equivalent of bromide of silver and chloride of sodium or potassium will be formed, and the

corresponding quantity of ammonia, which kept the chloride of silver in solution, will be free and act upon the bromide of silver; but by observing the before-mentioned precaution, every error of that kind will be avoided. Should a fluid contain iodides and bromides without chlorides, and not in too small a proportion, a very good method of ascertaining their respective quantities is to precipitate them at once with nitrate of silver, and to heat the dry precipitate in an atmosphere of bromine. I have found, when iodide of silver is melted in an atmosphere of bromine, it is entirely changed into a bromide; and from the difference of the weight between the mixture of iodide and bromide of silver, and that of the whole bromide of silver, the respective quantities of iodine and bromine may be ascertained. Thus the quantity of iodine (or bromine) stands in proportion to the difference of the weight, as the atomic weight of iodine (or bromine) is to the difference of their atomic weights. Hence it would only be required for the quantity of iodine to multiply the given difference of the weight by 2,627, and for that of bromine to multiply it by 1,627. Professor H. Rose, of Berlin, applies a similar method for the separation of iodine from chlorine.—(Poggendorff's *Ann.* 1834, No. 37, p. 583, 584.)

I may appear to have dwelt long upon this subject, but the importance into which brine springs have arisen on account of their powerful components, iodine and bromine, has induced me to examine the matter closely, as it may be of consequence to the medical profession to know the exact quantity of these valuable substances.

I have briefly to add, that the quantity of chlorine in sea-water was ascertained by means of nitrate of silver, deducting from it that proportion of bromine which had been found according to the foregoing method. The quantity of sulphuric acid was found by chloride of barium, the water having previously been mixed with a little nitric acid. Another portion of the water was mixed with chloride of barium without the addition of an acid, when the difference of the weight between this and the former precipitate gave the amount of carbonate of barytes from which the proportionate quantity of carbonic acid gas was computed; its quantity was likewise ascertained after the distribution of the acids amongst the bases, when the surplus of the lime or of one of the other bases must have been united to carbonic acid. The quantity obtained by analysis was a little less than the last, owing to the carbonate of barytes not being entirely insoluble in water during lixivation. Lime was separated by oxalate of ammonia, the water having been previously mixed with a proper

quantity of chloride of ammonium. After the separation of lime, magnesia was precipitated by the addition of ammonia and phosphate of ammonia.

The precipitate was washed with water containing 10 per cent. of ammonia, whereby the solution of the precipitate was prevented. After the sea-water had been freed from the earthy chlorides and sulphates by hydrate of barytes and carbonate of ammonia, it was evaporated to dryness, and the residue heated to redness, and weighed. The alkaline chlorides were dissolved in water mixed with perchloride of platinum, and evaporated to dryness. The residue digested with spirits of wine containing 60 per cent. alcohol, left potassio chloride of platinum, which was dried, weighed, and computed as chloride of potassium. The surplus of the total amount of the alkaline chlorides will give the precise quantity of the chloride of sodium.

The equivalent numbers have been computed according to the Tables which H. Rose has affixed to his *Handbuch der analytischen Chemie. Zweiter Band.*

I subjoin by way of comparison an analysis of the Mediterranean by Laurens. (*Journal de Pharmacie*, xxi. 93.)

	Sea-water of the British Channel. Grains.		Of the Mediterranean. Grains.
Water	964·74372		959·26
Chloride of sodium	27·05948		27·22
—— of potassium	0·76552		0·01
—— of magnesium	3·66658		6·14
Bromide of magnesium	0·02929		—
Sulphate of magnesia	2·29578		7·02
—— of lime	1·40662		0·15
Carbonate of lime	0·03301	Carb. of lime & magnesia.	0·20
	1000·00000		1000·00

When these analyses are compared, it will be found that the Channel water contains 9 times as much lime as the Mediterranean, but this can be accounted for, as the water flows over a bed of chalk. The Mediterranean again has twice as much magnesia and sulphuric acid.

We also find that the English Channel contains in 1000 grains water, 35·25628 grains of anhydrous ingredients; which amount corresponds very nearly to 35 grains, or 35·1 grains, obtained from several experiments, when 1000 grains were evaporated in a platina crucible, mixed with a little chloride of ammonium, to prevent as much as possible the decomposition of the earthy chlorides, and the residue care-

fully ignited, in order to volatilize the chloride of ammonium, where, however, a dissipation of hydrochloric acid had taken place.

Sometimes I found faint traces of oxide of iron, when the concentrated water was mixed with sulphocyanuret of potassium, particularly after boisterous weather; I found the same in respect to organic matter. The sea-water taken on a fair and calm day, when very transparent, did not yield the slightest indication of extractive matter when evaporated and ignited. A small quantity of free carbonic acid gas has been likewise found; and also extremely minute traces of chloride of ammonium were detected, when about 5 pounds of sea-water were evaporated in a water-bath to nearly half an ounce, which, mixed with caustic soda, produced fumes close to a glass rod wetted with hydrochloric acid.

Sea-water has been likewise examined for silica, alumina, strontia, manganese, phosphoric acid, and nitric acid, none of which could be detected.

The sea-water used for the occasion was taken on the 3rd of June, from the surface six miles from the shore, at high water. The weather was fair, the sea calm and extremely transparent. Its specific weight was at 60° Fahr. 1·0274. Another portion obtained by a proper apparatus from the very bottom of the sea, 10 fathoms deep, was of the same specific gravity, and likewise that taken almost close to the shore. In the month of July, after a previous rainy day, the sea-water taken four miles from the shore, had at 60° Fahr. a specific gravity of 1·0274; at a distance of 2 miles, 1·0271; and close to the shore, 1·0268. It was examined several times in August, the weather being fair and warm, when the specific gravity amounted to 1·0274. This appeared to be the greatest weight.

When weighed in fair weather in December, it was almost 1·0271; after rain I found it to be 1·0267. These variations will of course depend entirely on the state of the weather. If the atmosphere be bright, and no heavy rain has lately fallen, the water will have, even close to the shore, the same specific weight as out at sea, but after rain it is obvious that the sea-water close to the shore will be most diluted. It is therefore indispensable that the sea-water for examination should be taken at a distance of several miles, that its specific weight should be ascertained, and that the analysis should be performed from one and the same dip.

I cannot conclude this paper without drawing the attention of medical men to the importance which the brine springs on the Continent have lately acquired, as, for instance, the springs

near Kissingen, the Adelheids quelle, near Heilbroun, and above all, the springs of Kreugnach, which have been found highly beneficial in scrophulous diseases when internally administered, their action being dependent entirely on the chlorides, iodides, and bromides they contain. Sea-water would afford similar advantages for bathing, and when evaporated to dryness, the residue might be kept in earthen vessels, and thus be conveyed to any distance; and as its constituents are very soluble, sea-water in perfection might be procured at any place. The evaporation of sea-water should be performed with care, and the ingredients kept by chemists. One great advantage would accrue from this method, viz. that sea-water could be had of any degree of concentration which the practitioner might deem necessary. At the baths of Kreugnach, for example, extraordinary effects have been produced when from 40 to 70 quarts of the mother liquor were added to the natural salt-water of that spring, and this mixture used for bathing.

German Spa, Brighton, June 1839.

XI. *On the Use of the Galvanic Battery in Blasting. By* Hamilton K. G. Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Johnstone Castle, Wexford, May 24, 1839.

I BEG to trespass on your time by this letter on the use of the galvanic battery, instead of the fuze in blasting.

The papers have given short descriptions of the experiments made at Chatham, but all the details were not given. I commenced my experiments on blocks of the old trees that were blown down by the late storm. I first prepared an igniting cartridge by joining two pieces of *clean* copper wire to the extremities of a steel wire taken from the scratch brush, such as is made use of by gun-makers; this steel wire is fastened to the copper wires by waxed silk; the length of steel wire to be deflagrated is one-fourth of an inch; a piece of very slight wood is spliced to both copper wires to protect the steel wire from any accident—it makes the whole strong and more convenient to be introduced into the small cartridge, which is either a quill or a small paper tube. They are filled with fine powder, and made air-and water-tight, to prevent the powder from getting damp and rusting the steel wire; a second small piece of wood is then fastened to this small cartridge and the copper wires; one of the wires is bent over this piece of wood and brought up at an angle with the other up-

right wire. This is my exploding cartridge: it cannot be easily put out of order. The wires of the cartridge have only to be made bright before they are fastened, by twisting them round the positive and negative wires of the battery. I always place the cartridge deep in the hole made to receive the powder, in order that the pressure from the turnpeg may be taken off by the quantity of powder above it.

The wire I make use of is the common copper bell-wire. The battery is the old Wedgewood trough, with 4-inch plates, double coppers. I prevent the zinc plates from touching the copper by small pegs of wood passed through the four corners. Wooden troughs with moveable divisions were tried, but not with any good result. A wooden trough with the plates in a frame of wood with varnished paper between the copper was tried, but the porcelain trough far surpassed them. My first experiment was blasting single blocks; the effect was much better than when the fuze was used, in consequence of the clay being more firmly driven round the wires than it would be round the larger surface of the fuze. 2ndly, I selected two large blocks nearly in line; the first block was 43 feet from the battery; the second block 113 feet from the battery, and the blocks consequently were 70 feet from each other. On dipping the plates the explosions took place in quick succession; the battery consisted of 30 pairs of 4-inch plates. 3rdly, I wished to try the effect of a simultaneous explosion of two blasts on a very large block firmly tied together by rivets. The positive wires of each cartridge were fastened to the positive connecting wire, and in like manner the negative wire. The effects of this simultaneous explosion were very good; the exciting liquor being weak, the connecting wires were shortened to 98 feet. 4th, To amuse some friends, I exploded some powder in one of the ponds, depth 10 feet; length of wire 210 feet; 40 pairs of plates, with old exciting liquor:—the experiment succeeded to the delight of all; a large eel was killed by the blow-up. I have no doubt but that wild-fowl will yet be killed by means of shells placed at low water on the banks where they feed; and by means of long connecting wires, the shells can be made to explode simultaneously among the birds.

I find that 10 pairs of 4-inch plates free from oxide and charged with the following exciting liquor—water, $6\frac{1}{2}$ quarts; sulphuric acid of commerce, $4\frac{1}{2}$ ounces; nitrous acid, $4\frac{1}{4}$ ounces; will ignite powder with a wire 101 feet long. 20 pairs of plates ignited powder at the distance of 353 feet. I tried to repeat this experiment, but did not succeed, though the plates were

only three times immersed in the acid, and only for about two seconds each time. I tried the same battery at 268 feet, and did not succeed. The plates were then well washed, and fresh exciting liquor made: the experiment again failed; the plates were quite inactive. The next day I tried the same plates and the same exciting liquor, and succeeded at 268 feet. From this it seems impossible to say how many pairs of plates would be required to produce uniform effects at long distances. I suspect that the zinc plates do not act equally in producing the electricity, which causes this variation.

I should have liked very much to have tried the conducting properties of different sized wires, but had not an opportunity of getting them here.

Not having seen any notice of this novel and safe method of blasting in your excellent journal, induced me to send you these few remarks.

I believe I am the first in Ireland that applied the galvanic battery instead of the fuze in blasting.

I remain, Gentlemen, yours, &c.

HAMILTON K. G. MORGAN.

XII. *On the Equivalent of Carbon. By Mr.* GEORGE FOWNES.

To R. Phillips, Esq. F.R.S. &c.

SIR,

HAVING read with great interest an extract from the "*Journal de Pharmacie*," in your Number for February, on the composition of naphthalin, by M. Dumas, and the probability, in his opinion, of the existence of an error in the commonly received combining number of carbon, I beg to send you an account of some recent analyses of that substance, and also of three other compounds of hydrogen and carbon, made in the laboratory of the Middlesex Hospital Medical School: I must leave it to you to decide whether these experiments are of a proper kind to occupy a page of your Journal in the absence of more original and important matter.

The subject of the composition of naphthalin has engaged the attention of chemists ever since its discovery, and notwithstanding the care which has been bestowed on its analysis, the results differ more than ought to be expected from processes so accurate as those employed in the investigation. There is one circumstance, however, to be remarked in nearly all, namely, that the sum of the weights of the hydrogen and carbon obtained by experiment, always exceeds that of the sub-

stance employed. As an illustration may be adduced, three of the analyses lately made in the Giessen laboratory by M. Woskresensky, 100 parts of naphthalin gave

Carbon...	94,625	94,494	94,395
Hydrogen	6,528	6,526	6,206
	101,153	101,020	100,601

In repeating these experiments with great care by the aid of Professor Liebig's process, results differing little from the foregoing were obtained.

The specimen of naphthaline employed was purified by very slow sublimation in a Florence flask, by which means it was obtained in large crystalline plates of the most brilliant whiteness. In this state it had very little odour when cold, and melted on the application of heat to a colourless, transparent fluid, which crystallized confusedly on cooling:

(1)	4·338 grs. naphth. gave	14·82 grs. carb. acid and	2·45 water
(2)	4·944	16·93	2·78 —
(3)	4·87	16·67	2·72 —

	(1)	(2)	(3)
Carbon...	94·46	94·68	94·65
Hydrogen	6·27	6·25	6·21
	100·73	100·93	100·86

As the error of excess here to be observed exceeds the usual limit of inaccuracy of the experiment, no other supposition remains but that of M. Dumas, that the equivalent of carbon may be taken too high.

Bicarburet of hydrogen, the benzin of Mitscherlich, being apparently a substance of very definite nature, it was thought that some additional light might be obtained from an analysis of this liquid. It was carefully prepared in the manner described by its discoverer, by slowly heating to dull redness in a capacious earthen retort, a mixture of 4 ounces of crystallized benzoic acid and 12 of hydrate of lime. The condensed oily liquid being separated from the water on which it floated, agitated with fragments of caustic potash, and finally redistilled from a large quantity of recently fused and powdered chloride of calcium, after having stood some days in contact with that salt, had a specific gravity = ·885 at 60°; refracted light strongly, and boiled in a glass tube at 177° Fahrenheit. Burned with oxide of copper.

			Water.
(1) 5·548 grs. benzin	gave 18·59 grs. carb. acid. and		3·8 grs.
(2) 4·762	.... 16·01	....	3·26 grs.
(3) 6·652	.... 22·29	....	4·56 grs.

	(1)	(2)	(3)
Carbon ...	92·65	92·96	92·66
Hydrogen	7·61	7·61	7·62
	100·26	100·57	100·28

Two other commonly reported hydrocarbons, namely, native naphtha and oil of turpentine, were also examined after careful purification, but a deficiency instead of an excess was observed, supporting the opinion by some entertained, that these substances, freed from water, sometimes contain under ordinary circumstances oxygen.

100 parts of native naphtha gave

	(1)	(2)	(3)
Carbon ...	87·17	87·02	86·95
Hydrogen	12·82	12·79	12·79
	99·99	99·81	99·74

100 parts of oil of turpentive gave

	(1)	(2)	(3)
Carbon ...	87·71	87·72	87·78
Hydrogen	11·45	11·45	11·46
	99·16	99·17	99·24

The experimental results of these analyses were reduced by means of Liebig's table, in which the equiv. of carbon is taken = 6·1148.

The general mode by which the combining number of carbon has been determined, is, as is well known, founded on the circumstance, that when that substance unites with oxygen to produce carbonic acid, the volume of the oxygen suffers no change. Hence, on subtracting the specific gravity of the one gas from that of the other, the ratio in which the two bodies unite is at once seen.

The pairs of experiments mostly relied on for determining the specific gravity of these two gases are those of MM. Berzelius and Dulong, Biot and De Saussure. The equivalent of carbon, as deduced from each of these, is here given.*

Berzelius and Dulong	Biot.	De Sausure.
6·115	6·0314	6·096

As the names here mentioned must be considered of the very highest authority in science, it appears evident from the

* *Lehrbuch der Chimie.*

discrepancy of the results, that the method is not susceptible of sufficient accuracy for the present purpose.

In my own analyses of naphthaline, the carbon and hydrogen bear such a relation to each other, that assuming its constitution to be $C_5\ H_2$, the number for carbon, according to each experiment, will be

(1)		(2)		(3)
6·026	......	6·059	......	6·096

The mean of which gives 6·06.

It appears to me therefore on the whole, that the analysis of certain hydrocarbons offers a surer mode of settling the question at issue than a comparison of the densities of the two gases; and of all known substances of this kind, none seems better adapted to the purpose than naphthaline, inasmuch as its very peculiar physical and chemical character renders it a matter of no difficulty to obtain it in a state of complete purity. It is obvious that a considerable number of analyses, made under different circumstances, and with various samples of the substance, will be necessary in order to obtain an approximation sufficiently good to be relied on.

In the experiments above related the usual means were adopted to guard against the besetting error of organic analysis—the tendency of the oxide of copper to attract moisture from the air, and so render the hydrogen estimation worthless. I am indebted to the kindness of Mr. Everitt for the use of the instruments employed, and take this opportunity of expressing my gratitude.

I remain yours most respectfully,

Geo. Fownes.

6, Coventry Street, April 5, 1839.

XIII.—*On a Leather-like Substance found formed upon a Meadow.* By Charles Kersten, Prof. of Chemistry in Freiberg, Saxony, and Prof. Ehrenberg, of Berlin*.

INCLOSED I send you an interesting vegetable production, having a deceptive resemblance to white dressed glove-leather, and which was found by M. Lindner on a meadow above the wire-factory at Schwartzenberg in the Erzgebirge.

A green slimy substance grew on the surface of the stagnant waters in the meadow, which, the water being slowly let off, deposited itself on the grass, dried, became quite colourless, and might then be removed in large pieces. The outside

* We are indebted for this communication, and for a specimen of the substance described, to the kind attention of Prof. Kersten. The original appeared in Poggendorff's 'Annalen,' Part I. 1839.—R. T.

of this natural production, as you will observe, resembles soft dressed glove-leather, or fine paper, is shining, smooth to the touch, and of the toughness of common printing-paper*. On the inner side, which was in contact with the water, it has a lively green colour, and one can still distinguish green leaves, which have formed the leather-like pellicle. I dare say a botanist could still determine the *species* to which they belong.

I have made the following experiments on the leather-like substance, having separated it from the green inner coat.

It catches fire very easily, burns with a wax-yellow flame, leaving a pale-red rough light ash. When heated in a small retort dense white fumes are evolved, an odour of burnt paper is perceived, and simultaneously drops of a yellow empyreumatic oil are deposited on the neck of the retort. Somewhat later, water, having a strong acid action, is given off, which evaporates without leaving any residue. A light charcoal remains in the bottom of the retort.

Water, alcohol, æther, nitric acid and aqua regia have no action on it, nothing being dissolved, nor does its texture alter when heated with these re-agents. A solution of hydrate of potassa dissolves it to a brown slimy fluid; caustic ammonia has at first only a slight action, but after some days it swells out, becomes like wet printing paper, and is partially altered.

If the substance is gradually heated with hydrate of potassa, and the gas then given off conducted into a solution of nitrate of mercury, there is *no* black precipitate, neither are white fumes observable when the gas is brought into contact with a glass rod dipped in acetic acid: consequently *no ammonia is formed when the substance is burnt*, and, therefore, *it can contain little or no nitrogen.* The ash of itself, or when moistened with sulphuric acid, does not colour the oxidizing flame of the blowpipe. In borax it is dissolved, giving a gloss which while warm is of a deep yellow, when cold of a pale yellow colour. With the double salt phosphate of soda and phosphate of ammonia it gives a pale yellow glass, leaving a thin scale of silica. Fused with soda and saltpetre on a platinum plate the ash gives a deep green mass. It has no alkaline action, does not effervesce with acids, nor does it contain any salt soluble in hot or cold water.

Thus the ash of the substance in question is composed essentially of silica, oxide of manganese and oxide of iron. The substance itself appears to be an aggregation of leaves, from which the green colouring matter, the extractive matter, and also the organic matters have by some organic process entirely disappeared.

* Unsized paper.—Edit.

I shall endeavour to find out the circumstances under which this complete discoloration of the ligneous matter of the leaves takes place, for this is interesting in a technical point of view.

Postscript by Dr. Ehrenberg.

The very meritorious attention of Prof. Kersten to this leather-like substance has recalled to my mind the subject formerly touched upon p. 119 of my work on Infusoria, but particularly so in relation to the meteoric paper of Courland * that I could not omit submitting it to a microscopic examination. With regard to this meadow-leather of Schwarzenberg, it consists most distinctly of *Conferva capillaris*, *Conferva punctalis* and *Oscillatoria limosa*, forming together a compact felt, bleached by the sun on the upper surface, and including some fallen tree leaves and some blades of grass. Among these confervæ lie scattered a number of siliceous infusoria, chiefly *Fragilariæ* and *Meridion vernale*. I have observed sixteen different sorts of such siliceous infusoria, belonging to six genera; besides these I have found three sorts of infusoria with membranous shields, and dried specimens of *Anguillula fluviatilis*.

Thus the silica is quite explained, as well as a part of the iron, of which last another part, as also the manganese, may arise from a little dust which lies in irregular particles with the infusoria among the confervæ. I have treated more circumstantially, before the Academy of Sciences, of the meteoric paper of 1686, which I found to be similar to this in composition.

XIV. *Notices respecting New Books.*

Chemistry of Organic Bodies. Vegetables. By THOMAS THOMSON, *M.D., F.R.S., &c., and Regius Professor of Chemistry in the University of Glasgow.*

THE object of the present volume, we are informed by its author, is to lay before the British chemical public a pretty full view of the present state of the chemistry of vegetable bodies; and he truly observes, that "this branch of the science has made so much progress of late years, that a very wide and inviting field has been laid open. Several hundred new substances have been either discovered, or their characters have been determined with such precision, and their composition investigated with such accuracy, as to give a pretty accurate idea of their constitution, and of their connexion with each other. These ultimate analyses, with very few exceptions, have been all made upon the continent, and chiefly in Germany and in

* A translation of Dr. Ehrenberg's notice on the meteoric paper of 1686 has appeared in the Annals of Natural History, vol. iii. p. 185.

France. British chemists have hardly entered on the investigations."

In the compilation of this work, Dr. Thomson has exerted much patient industry; for as the analyses were chiefly made in foreign countries, it required for its completion numerous references, not merely to French works which are in the hands of many English chemists, but to German works, which are much less readily obtained, and less perfectly understood.

Some idea of the extent of the information contained in this work may be formed from merely stating the fact, that it consists of more than 1000 closely-printed octavo pages. It consists of four divisions: 1st. Vegetable principles, comprehending the vegetable acids and vegetable alkalis, intermediate bodies, oils, resins, &c. &c.; the 2nd division includes the parts of plants; the 3rd treats of vegetation; and the last, of the decomposition of plants.

Dr. Thomson, by thus collecting the numerous facts relating to vegetable bodies into one volume, has performed a most useful and laborious task. We are not aware of the existence of any similar work, even on the continent; and the English chemist had to search, and not always with success, through a mass of foreign journals, for the knowledge which he will now readily acquire; and we strongly recommend it to such as wish to acquire information on the important branch of chemistry to which it is dedicated. The mode in which the work has been conducted will be readily appreciated by the selection which we have made of the chapter on Pyroxylic Spirit for a separate article, a part of which appears in our present number.

Mémoire sur l'Irradiation par J. Plateau, Membre de l'Académie Royale des Sciences de Bruxelles, Professeur à l'Université de Gand, &c.—Bruxelles, 1839.

[Extrait du tome XI. des *Mémoires de l'Acad. Roy. des Scienc. de Bruxelles.*]

A copy of the above memoir has just reached us, the Report upon which to the Royal Academy of Brussels at its sitting Aug. 5, 1837, we here insert.

Optics.—M. Plateau has presented to the Academy a memoir on irradiation. "The object which I have proposed to myself in this memoir," says the author, "is to put an end to the uncertainties which still exist amongst astronomers and physicists as to the very existence of irradiation, and to examine the causes of the phænomenon, its influence in astronomical observations, and the laws to which it is subject, rather more closely than has hitherto been done.

"Irradiation is the phænomenon by virtue of which a luminous object surrounded by an obscure space appears more or less amplified. It is usual to cite as an example the appearance which the moon exhibits when she is seen in the form of a crescent, and at the same time allows the remainder of her disk to be distinguished, feebly illuminated by the ash-coloured light. The exterior contour of the luminous part then seems to present a marked projecture on that of the obscure part; in other words, the crescent appears to

form part of a disk very perceptibly larger than that to which the remainder of the moon belongs.

"This apparent encroachment of the edge of a luminous object upon the obscure space which surrounds it, carries with it an illusion of an opposite kind, with regard to an obscure object projected upon a luminous field. The dimensions of this object then appear to be diminished: for the irradiation produced along its outline by the surrounding luminous field extends within this outline.

"It is needless to insist upon the importance of irradiation in astronomy. An illusion which tends to increase the apparent dimensions of luminous objects projected upon an obscure ground, and to diminish those of obscure objects projected upon a luminous field, must, it should seem, have an effect more or less decided upon all observations the object of which is the measurement of the apparent diameters of celestial bodies, eclipses, the transit of planets over the sun, &c. The phænomenon has accordingly, in an especial manner, exercised the sagacity of astronomers. But what is singular is, that the observations present in this respect the greatest divergence; some seem to show a marked influence of irradiation, others appear completely exempt from the errors which accompany it. Hence also a divergence of opinion among astronomers relative to the very existence of the phænomenon, some of them admitting this existence, and others calling it in question. It is therefore important to seek out the truth amidst all these doubts, and to determine the causes which must have given them birth. I think I establish beyond a doubt that irradiation really exists, that it is one of the phænomena of vision most easily proved, and that if, in the observations made through astronomical instruments, it has sometimes ceased to manifest its influence, it is from causes that may be accounted for.

"On the other hand, several theories have been proposed in succession to explain the cause of irradiation. One of these theories, although very ancient, is generally adopted. It consists in admitting that the impression produced at the bottom of the eye by a luminous object, spreads upon the retina to a little distance all around the space directly excited by the light, so that the total sensation corresponds at that time to an image rather larger than the true one. This simple theory has however met with opponents, and a different explanation has very recently been put forward. I examine the different hypotheses proposed, and I endeavour to support by new proofs the one whose principle I have just mentioned.

"The phænomenon is indeed governed by remarkable laws, which may lead to processes adapted for guaranteeing the observations of its influence. Besides the laws already known, experience has led me to admit some new ones. I examine all of them theoretically, and I point out simple processes for verifying them.

"The principal results of my inquiries may be summed up in the following manner:

"1. Ocular irradiation is perceptible at all distances, from any separation whatever to the shortest distance of distinct vision.

"2. Ocular irradiation increases with the continued contemplation of the object.

"3. Two ocular irradiations near one another, which tend to act in a contrary direction, and to encroach upon each other, mutually destroy each other, and the more completely the nearer they are made to approach.

"4. Ocular irradiation varies considerably in different persons.

"5. In observations made through astronomical glasses, that part of the total error which arises from ocular irradiation is dependent upon the enlargement on the brightness of the image, and on the greater or less degree of sensibility of the eye of the observer for irradiation.

"6. This part of the total error necessarily vanishes in observations where a double image micrometer is used.

"7. The part of the total error attributable to the aberrations of the glass necessarily varies with different instruments, but for the same glass it may be considered as constant, that is to say, independent of the magnifying enlargement.

"8. The irradiation in glasses, or the total error arising from the ocular irradiation and from the aberrations of the instrument, is necessarily variable, since it depends upon variable elements."

XV. *Proceedings of Learned Societies.*

LINNÆAN SOCIETY.

[Continued from vol. xiv. p. 463.]

March 5.—READ, "Observations on some Fungi or Agarici, which by deliquescence form an inky fluid, drying into a bister-coloured mass, capable of being used as a water-colour for drawings, and of a very indestructible nature by means of common agencies." By John Redman Coxe, M.D., formerly Professor of Materia Medica in the University of Pennsylvania. Communicated by the Secretary.

Dr. Coxe having gathered a Fungus and placed it on a sheet of white paper, leaving it until the next day, found several drops of an inky fluid, slowly trickling from the inner surface, which had assumed a black appearance; by placing the Fungus in a glass, the whole except the outer skin liquefied. The colour of the fluid was rather a deep bister than black, and being left in the glass, in a few hours it separated into a solid sediment, with a lighter coloured fluid swimming above. Having afterwards collected a considerable quantity of fluid from the same species, he obtained by drying an extract of a pretty deep black colour of both parts conjoined, which would otherwise have separated. This on trial formed an admirable bister-like water-colour, well adapted for drawing when mixed with a little gum.

Dr. Coxe used the "fresh inky fluid as ink, and from such fresh fluid the accompanying drawings were made;" but it was soon found that its change was too rapid to think of depending on it for such a purpose, he therefore was led to dry it as quickly as possible by spontaneous evaporation, and then to use it diluted with water. Having exposed various portions of writing thus made to the direct rays of the sun for several months with little change, he tried the effects of chlorine and euchlorine gas, muriatic acid, and ammoniacal

gases: from these but a trifling change ensued, except from the muriatic acid gas, which destroyed very considerably the dark tint of the writings. He also placed some small and recent specimens of the Fungus in a solution of corrosive sublimate, which preserved them and prevented any deliquescence: the same effect was produced by alcohol.

The ink is fully formed and escapes in about three or four days. When received into a phial, in a short time the heavier and blacker matter was found to settle as a sediment; the lighter brownish amber-coloured fluid surmounts it, and may be poured off from it to dry them separately. From a good-sized specimen nearly half an ounce of fluid has been obtained.

The following chemical experiments among others were made:—

1. Two drachms of the fluid added to ℥ 1 of hydrate gave a clear brown transparent solution, to which in separate glasses was added

2. *Nitrate of Silver*: no effect at first, but in a few minutes dark brown flocculi subsided, leaving a transparent fluid above.

3. *Muriate of Barytes*: no effect at first, finally a subsidence of dark brown flocculi.

4. *Acetate of Lead.* Immediate dark brown flocculi, leaving a clear liquid above.

5. *Carbonate of Potash.* Transparency destroyed; a trifling brown deposit in a few hours.

6. *Alcohol.* No apparent change from it.

7. *Solution of Corrosive Sublimate.* An apparent diffusion of brownish hue, gradually subsiding in dirty brown flocculi.

8. *Dilute Muriatic Acid.* The same, but much smaller in amount.

10. *Lime Water.* Light brown flocculi in a few hours.

11. *Liquor Ammoniæ.* No effect.

12. *Succinate of Ammonia.* Deep brown deposit in a few hours.

13. *Prussiate of Potash.* No effect.

14. *Oxalate of Ammonia.* Clouds form and settle in a dirty brown sediment.

From these experiments Dr. Coxe is disposed to think that an excellent *India Ink* might be prepared for drawing; perhaps its dried deposit mixed with oil might answer for engravings; and as an ink, indestructible from any common agents, it might be well to try it in the filling up of bank notes and other papers of consequence, as he believes it cannot be extracted by any means without destroying the paper itself.

The Fungus described, and on which the above experiments were tried, is referred with some hesitation to *Agaricus ovatus*, Schæffer, 'Icones Fungorum,' fig. 7. *A. cylindricus*, fig. 8. *A. porcellaneus*, fig. 46. and 47. The drawings are named *Agaricus ovatus**.

* The drawings evidently represent *Agaricus fimetarius*, Linn. and Curtis; *A. comatus*, Mull. and Berkeley; *A. cylindricus*, Sowerby; to which *A. cylindricus*, Schæff. f. 8. and *A. porcellaneus*, figs. 46 & 47. belong; it is not so clear that *A. ovatus*, fig. 7. (the name adopted by Dr. Coxe) does. In the subgenus named by Berkeley Coprinus every species is deliquescent. Curtis observes, under his *A. ovatus*, which is *A. atramentarius*, Bull. and Berk., that the seeds may be seen in the black liquor if magnified.

Mar. 19.—Read, "A Notice of the Birds of Iceland, accompanied by specimens." By George Townshend Fox, Esq., F.L.S.

Read also a paper, "On the Structure and Development of the Reproductive organs of *Pilularia globulifera*." In a letter to R. H. Solly, Esq., F.R.S. and L.S. By William Valentine, Esq., F.L.S.

April 2.—Mr. Owen read a Paper on a New Species of the genus *Lepidosiren* of Fitzinger and Natterer. The author commenced by adverting to the first announcement of that anomalous animal, the *Lepidosiren paradoxa*, as the type of a new genus of Perennibranchiate Reptiles by Fitzinger at the meeting of the German naturalists at Prague in 1837, and to its subsequent description by its discoverer Dr. Natterer, the well-known South American traveller.

With the generic characters assigned by these able German naturalists to their *Lepidosiren*, the species described by Mr. Owen fully and closely agreed; but it differed specifically in the greater relative length of the head and rudimental extremities, and its much smaller size.

Mr. Owen observed, that since the time of the discovery of the *Ornithorhynchus* there had not been submitted to naturalists a species which proved more strongly the necessity of a knowledge of its whole organization, both external and internal, in order to arrive at a correct view of its real nature and affinities, than did the *Lepidosiren*, and as he had felt a reluctance to bring before the Society an incomplete description, which might only have served to raise new doubts in the minds of naturalists with regard to this animal, he had deferred since June 1837 the completion and communication of the present paper. He had however at that time prepared a brief description of the specific characters of the specimen in question, under the name of *Protopterus*, and had referred it in the Catalogue of the Museum of the College of Surgeons to the Class of Fishes, on account of its scaly covering and the condition of its nostrils as plicated sacs, and to the abdominal family of the Malacopterygian order of that class, in which it seemed to present an extreme modification or rudimental condition of the fins indicative of a transition from the abdominal to the apodal families.

The anatomical details which formed the principal part of the present communication, confirmed the propriety of referring the *Lepidosiren* to the class of fishes; but they also led, Mr. Owen observed, to a considerable extension in his original views of its affinities in that class.

A minute description was then given of the external characters and peculiarities of the present species, which differed from the *Lepidosiren paradoxa* in the greater relative length of the head and rudimental fins as compared with that of the trunk; and in its general size, which is three-fourths smaller.

The chief peculiarities of the *skeleton* consist in its imperfect, or rather partial ossification, and in the green colour of the ossified parts; in which it resembles that of the gar-pike (*Belone vulgaris*). The parts which continue permanently in the cartilaginous condition are the petrous elements of the temporal bones containing the acoustic labyrinth, a portion of the articular pedicle of the lower jaw, the

branchial arches, and the bodies of the vertebræ: these, moreover, are not separated to correspond with the neurapophyses and ribs, as in Plagiostomous Cartilaginous Fishes, but retain their primitive confluent condition as a round continuous chord, extending from the occiput to the end of the tail: this vertebral chord consists of an external firm, elastic, yellowish capsule, enveloping a softer subgelatinous material, as in the Cyclostomous Fishes. The corresponding parts or basilar elements of the cranial vertebræ were ossified: and Mr. Owen then entered upon a detailed description of the skull.

The ribs are thirty-six pairs, and consist of short, slightly curved, slender styles, encompassing, with the spine, about one-sixth part of the cavity of the abdomen. The rudimental filiform pectoral and ventral fins were supported each by a single cartilaginous ray composed of many joints.

The muscles of the head, jaws, hyoid and branchial apparatus were then described: the muscular system of the body consists of subvertical layers of oblique fibres separated at brief intervals by aponeurotic intersections.

The following peculiarities of the *Digestive system* were then pointed out;—two long, slightly curved, slender, sharp-pointed teeth project from the intermaxillary bones, which are moveable. The upper maxillary bones support each a single dental plate divided into three cutting lobes, by two oblique notches entering from the outer side: the lower jaw is armed with a single dental plate similarly modified, the produced cutting edges fitting into the notches above: these maxillary teeth somewhat resemble the dental plate of the extinct *Ceratodus* of Agassiz. The fleshy and sensitive parts of the tongue are more developed than in fishes generally. The jaws are adapted to minutely divide and comminute alimentary substances; the pharyngeal opening is contracted; the entrance to the pharynx guarded by a soft semicircular valvular process. Gullet short, straight, narrow, but longitudinally plicated. Stomach simple, straight, with thick walls, in capacity corresponding with the œsophagus; terminating by a valvular pylorus projecting with a scalloped margin into the intestine. No pancreas or spleen. Liver well-developed, partly divided into two lobes. A gall-bladder, and large ductus choledochus, opening by a valvular termination close to the pylorus. Intestine round, straight, at first of equal diameter with the stomach, but gradually contracting to the vent, with thick parietes; traversed internally by a spiral valve describing six gyrations; the first of which is the longest.

The *respiratory organs* consist of branchiæ, and a double elongated air-bladder, with the usual vascular and cellular structure of the lungs of a reptile.

The *branchiæ* consist of elongated, sub-compressed, soft, pendulous filaments, attached to cartilaginous branchial arches; these arches are not joined together, or to the os hyoides by an intermediate chain of cartilages or bones below, nor are they articulated to the cranium above. There are six branchial arches on each side, and five intervals for the passage of the water from the mouth to the

branchial sac. All the branchial arches do not support branchial filaments; but only the first, fourth, fifth, and sixth.

The *heart* is situate below the œsophagus, in a strong pericardium; it consists of a single auricle and ventricle and a contorted bulbus arteriosus, with a longitudinal valvular process as in the *Siren*. The two branchial arteries, which wind round the gill-less arches, afterwards unite together on each side, and give off branches which form the pulmonary arteries, or those which go to the air-bladders.

The apparatus for aerial respiration commences by a short, single, wide and membranous trachea, or *ductus pneumaticus*, which commences by a longitudinal laryngeal slit, one line in extent, situated three lines behind the orifice of the pharynx: a single plate of cartilage is continued from this laryngeal opening forwards to that of the pharynx: the plate is as broad as the floor of the pharynx, and its office seems to be to prevent the collapse of the parietes of that tube, and to keep a free passage for the air to the trachea. This tube dilates at its lower end into a sac with very thin parietes, which communicates directly with each division or lobe of the air-bladder. These lobes or *lungs* are partially subdivided into small lobes at their anterior and broadest part; and then continue simple and flattened, gradually diminishing to an obtuse point situated behind the posterior extremity of the cloaca. The whole of the parietes of the lungs is honey-combed: the cells are largest, deepest and most vascular and subdivided at the anterior and broader end of the lung.

The two *kidneys* are quite distinct, very long and narrow, but broadest towards the cloaca: the ureters communicate with the back part of the common termination of the oviducts.

The *ovaria* are two long, flattened bodies, with ovisacs and ova of different sizes: many between 2 and 3 lines in diameter, scattered among clusters of other ova of smaller size. The *oviducts* are distinct tortuous tubes, which commence by a very wide and thin-coated portion, opening by a slit, 3 lines wide at their anterior extremity, and not communicating with each other before opening into the peritoneal cavity, as in the Plagiostomes.

A small *Allantois* is situated between the oviduct and rectum. The cloaca receives the above parts in the following order,—first, or most anteriorly, the common opening of the peritoneal canals; secondly, the anus; thirdly, the Allantoid bladder; fourthly, the oviducts, with the ureters, which open into the back part of the oviducts.

The *brain* consists of two elongated subcompressed distinct cerebral hemispheres; a single elliptical optic lobe, or representative of the bigeminal bodies; a simple transverse cerebellar fold, not covering the widely-open fourth ventricle; largely developed pineal and pituitary glands; and a single corpus mammillare.

The *nerves* given off from the brain, were the olfactory; the optic, which arose from the same point at the middle line between the crura cerebri, and did not decussate; the fifth pair; the acoustic; the pneumogastric; and lingual nerves: there were no traces of the third, fourth, or sixth nerves; there being no muscles to the eyeballs.

The *eyes* are very small, and adhere to the skin, which passes over

them without forming any projection; they have a small spherical lens, and no choroid gland.

The *organ of hearing* consists of a vestibule enclosed in a thick cartilaginous case, without external communication except for the foramina transmitting the *portio mollis*: it consists of two large otolithic sacs, containing each a white chalky mass; the external one being six times the size of the one next the brain: above these sacs are three small semicircular canals. No trace of tympanic cavity or *Eustachian tube*.

The *organ of smell* consists of two oval membranous sacs, plicated internally, and having each a single external aperture upon the upper lip; but without any communication with the mouth,—a structure which the author observed was perhaps the only single character which unexceptionably proved the *Lepidosiren* to be a true fish. The remaining evidence of its ichthyic nature reposed rather upon the concurrence of many less decisive characters.

These characters were stated to be, its covering of large round scales; the mucous ducts of the head and lateral line; the many-jointed soft ray supporting the rudimental pectoral and ventral fins; the gelatinous vertebral chord, united anteriorly to the whole of the basi-occipital, and not to two condyles as in Batrachia; a præopercular bone, the intermaxillary bone being moveable; the lower jaw having each ramus composed simply of a post-mandibular and dentary piece; the double row of spinous processes, both above and below the vertebral chord; the green colour of the ossified parts of the skeleton; the straight intestine, with its spiral valve; the absence of pancreas and spleen; the single peritoneal outlet; the position of the anus; the single auricle of the heart; the number of branchial arches, and the internal position of the gills; a long lateral nerve; acoustic labyrinth with large otolithes. These characters, with the nasal sacs opening only externally, prove satisfactorily the *Lepidosiren* to be a true Fish, and not a Perennibranchiate Reptile.

In the class of fishes, Mr. Owen pointed out the interesting relations of the *Lepidosiren* as a link connecting the Cartilaginous fishes with the Malacopterygians, and especially with the *Sauroid* genera, *Polypterus* and *Lepidosteus*, and at the same time making the nearest approach in the class of fishes to the Perennibranchiate Reptiles.

For the species here described Mr. Owen proposed the name of *Lepidosiren annectens*. It is a native of the river Gambia, Africa.

FRIDAY-EVENING MEETINGS AT THE ROYAL INSTITUTION.

April 12th.—Mr. Everitt on Professor Liebig's method of analysing organic bodies.

April 19.—Mr. Cottam on the manufacture of bricks and draining tiles by machinery.

April 26th.—Mr. J. T. Cooper on his instrument for measuring altitudes, the baroscope.

May 3.—Mr. S. Solly on the structure of birds in relation to their habits of life and position in the animal kingdom.

May 10.—Mr. Faraday. Some general remarks on flame.

May 17th.—Mr. Griffiths on the manufacture of working of glass.

May 24th.—Mr. Carpmael on the manufacture of covered buttons.

May 31st.—Mr. Snow Harris on the laws and nature of electrical attraction.

June 7th.—Mr. Faraday on Hullmandell's mode of producing designs and patterns on metallic surfaces. Conclusion.

XVI. *Intelligence and Miscellaneous Articles.*

NEW EXPERIMENTAL RESEARCHES ON CAOUTCHOUC. BY ANDREW URE, M.D., F.R.S. &c.

THE specific gravity of the best compact *Para* caoutchouc, taken in dilute alcohol, is		0·941567
The specific gravity of the best	Assam is	0·942972
,, ,,	Sincapore	0·936650
,, ,,	Penang	0·919178

In the process of making the ELASTIC TISSUES*, the threads of caoutchouc are first of all deprived of their elasticity, to prepare them for receiving a sheath upon the braiding machine. For this purpose they are stretched by hand, in the act of winding upon the reel, to 7 or 8 times their natural length, and left two or three weeks in that state of tension upon the reels. Thread thus *inelasticated* has a specific gravity of no less than 0·948732; but when it has its elasticity restored, and its length reduced to its pristine state, by rubbing between the warm palms of the hands, the specific gravity of the same piece of thread is reduced to 0·925939. This phænomenon is akin to that exhibited in the process of wire-drawing, where the iron or brass gets condensed, hard, and brittle; while it disengages much heat; which the caoutchouc thread also does in a degree intolerable to unpractised fingers, as I have experienced.

Having been favoured by Mr. Sievier, managing director of the Joint-stock Caoutchouc Company, and by Mr. Beale, engineer, with two different samples of caoutchouc juice, I have subjected each to chemical examination.

That of Mr. Sievier is greyish brown, that of Mr. Beale is of a milky grey colour; the deviation from whiteness in each case being due to the presence of aloetic matter, which accompanies the caoutchouc in the secretion by the tree. The former is of the consistence of thin cream, has a specific gravity of 1·04125, and yields, by exposure upon a porcelain capsule in a thin layer, for a few days, or by boiling for a few minutes, with a little water, 20 per cent. of solid caoutchouc. The latter, though it has the consistence of pretty rich cream, has a specific gravity of only 1·0175. It yields no less than 37 per cent. of white, solid, and very elastic caoutchouc.

It is interesting to observe how readily and compactly the separate little clots or threads of caoutchouc coalesce into one spongy mass in the progress of the ebullition, particularly if the emulsive mixture be stirred; but the addition of water is necessary to prevent the coagulated caoutchouc from sticking to the sides or bottom

* See "Dictionary of Arts, Manufactures, and Mines."

of the vessel and becoming burnt. In order to convert the spongy mass thus formed into good caoutchouc, nothing more is requisite than to expose it to moderate pressure between the folds of a towel. By this process the whole of the aloetic extract, and other vegetable matters, which concrete into the substance of the balls and junks of caoutchouc prepared in Assam and Java, and contaminate it, are entirely separated, and an article nearly white and inodorous is obtained. Some of the cakes of American caoutchouc exhale when cut the fœtor of rotten cheese; a smell which adheres to the threads made of it, after every process of purification.

In the interior of many of the balls which come from both the Brazils and East Indies, spots are frequently found of a viscid tarry-looking matter, which, when exposed to the air, act in some manner as a ferment, and decompose the whole mass into a soft substance, which is good for nothing. Were the plan of boiling the fresh juice along with its own bulk of water, or a little more, adopted, a much more valuable article would be obtained, and with incomparably less trouble and delay, than has hitherto been brought into the market.

I find that neither of the above two samples of caoutchouc juice affords any appearance of coagulum when mixed in any proportions with alcohol of 0·825 specific gravity; and, therefore, I infer that albumen is not a necessary constituent of the juice, as Mr. Faraday inferred from his experiments published in the 21st vol. of the Journal of the Royal Institution.

The odour of Mr. Sievier's sample is slightly acescent, that of Mr. Beale's, which is by far the richer and purer, has no disagreeable smell whatever. The taste of the latter is at first bland and very slight, but eventually very bitter, from the aloetic impression upon the tongue. The taste of the former is bitter from the first, in consequence of the great excess of aloes which it contains. When the brown solution which remains in the capsule, after the caoutchouc has been separated in a spongy state by ebullition from 100 grains of the richer juice, is passed through a filter and evaporated, it leaves 4 grains of concrete aloes.

Both of these emulsive juices mix readily with water, alcohol, and pyroxylic spirit, though they do not become at all clearer; they will not mix with *caoutchoucine* (the distilled spirit of caoutchouc), or with petroleum-naphtha, but remain at the bottom of these liquids as distinct as mercury does from water. Soda caustic lye does not dissolve the juice; nitric acid (double aquafortis) converts it into a red curdy magma. The filtered aloetic liquid is not affected by the nitrates of baryta and silver: it affords with oxalate of ammonia minute traces of lime.—*London Journal of Arts, March,* 1839.

ON THE PREPARATION OF FULMINIC ACID. BY M. FEHLING.

Fulminic acid is one of those which have not hitherto been obtained in an isolated state. When separated from its compounds by a stronger acid, it decomposes rapidly, and its elements form new products by uniting in different proportions.

MM. Gay Lussac and Liebig have unsuccessfully endeavoured to isolate this compound. The decomposition of fulminate of silver by

the hydrochloric and hydrosulphuric acid, gave rise to new acids containing chlorine or sulphur; oxalic and sulphuric acids decomposed it with the disengagement of prussic acid. No compounds of fulminic acid and the alkalis, potash and soda, or barytes, &c., have been obtained. In all the experiments performed with the intention of preparing these compounds, double combinations have always been obtained, such as that of barytes with oxide of silver or of copper, and these soon decomposed when attempts were made to separate the oxide of silver or of copper.

Sometime since, however, Mr. E. Davy described a method for the preparation of fulminic acid; according to his process it may be obtained in combination with water, though only for a short time. He prepares a pure solution of fulminic acid, by adding hydrate of barytes to fulminate of zinc. This addition is to separate the oxide of zinc, and to yield pure fulminate of barytes, which is readily decomposed by the addition of dilute sulphuric acid. By the invitation of M. Liebig, M. Fehlen undertook to repeat the experiments of Mr. Davy. For this purpose, fulminate of protoxide of mercury was digested for some time with an excess of metallic zinc; by this a solution of pure fulminate of zinc was obtained, free from any trace of mercury. This solution treated with hydrochloric acid yielded a very strong smell of hydrocyanic acid, and at the same time that of cyanic acid was also perceptible. The salts of silver precipitated this solution: the white precipitate obtained in abundance was completely dissolved in boiling water; when dried it detonated strongly. If the solution of fulminate of zinc be cautiously evaporated by a water bath, a yellow powder is obtained, but a small portion of which is soluble in water; the aqueous solution gives a light white precipitate with the salts of silver, which does not detonate. The portion which is insoluble in water dissolves with effervescence in acids, and hydrocyanic acid is evolved; if the yellow powder be heated alone, it becomes white, oxide of zinc is obtained, and ammonia evolved. The substance, which gives a lemon yellow with the oxide of zinc, was not more minutely examined.

Solution of barytes was added to some recently prepared fulminate of zinc, until the solution was strongly alkaline: a large quantity of oxide of zinc was separated; the excess of barytes was precipitated by carbonic acid. The solution separated by filtration from the carbonate of barytes, acted with the salts of silver and with acid like fulminate of zinc; thus it yielded with the salts of silver fulminate of silver, &c. If sulphuric acid be added to the solution of the fulminate, all the barytes is precipitated. The filtered liquor, which, according to Mr. Davy, ought to be pure fulminic acid, when rendered alkaline by ammonia, gives nevertheless an abundant precipitate of sulphuret of zinc when treated with hydrosulphate of ammonia; the presence of oxide of zinc was also proved to exist in the liquor by carbonate of soda; the precipitate obtained by it when heated by the blow-pipe exhibited all the properties of oxide of zinc. Barytes, therefore, precipitates only a portion of the oxide from fulminate of zinc: there exists therefore, in solution a double salt, composed of fulminic acid, oxide of zinc, and barytes; the sulphuric acid

separates the barytes, and there is thus obtained acidulous fulminate of zinc, but not pure fulminic acid.—*Journal de Pharmacie*, Jan. 1839.

BIRTH OF A GIRAFFE AT THE GARDEN OF THE ZOOLOGICAL SOCIETY.

The following particulars of the birth of the young Giraffe were communicated to the Zool. Soc. at the Meeting on Tuesday evening last by Professor Owen.

"The Giraffe brought forth a young male June 19th, after a gestation of 15 lunar months. The young animal was able to stand a few hours after birth, and could reach the height of six feet. He was capering about the day after he was born, and shows a remarkable degree of development and strength, as might be expected from the long period of gestation. The mother, though not unkind to her offspring, refuses to suckle him; but there seems to be no difficulty in bringing him up by hand. Admeasurements of the different parts of the young animal were given, and the anatomy of the fœtal membranes and cotyledons described. Drawings of the mother and her young, by Mr. Hills, the well-known animal painter, were exhibited to the meeting."

METEOROLOGICAL OBSERVATIONS FOR MAY, 1839.

Chiswick.—May 1—6. Very fine. 7. Clear and dry. 8. Fine: much thunder and lightning at night. 9, 10. Cloudy and cold. 11. Fine, but cold. 12. Cloudy: rain. 13. Clear. 14. Cold rain. 15, 16. Clear: cloudy and cold: frosty at night. 17. Fine: frosty at night. 18. Very fine. 19. Overcast. 20. Very fine. 21. Dry haze. 22—25. Cold and dry. 26. Dry haze: fine. 27—31. Fine.—The weather at the commencement of the month was very fine, but after the thunder on the 8th it became cold and unseasonable. The nights were generally cold, and between the 14th and 17th they were successively frosty.

Boston.—May 1. Fine. 2. Cloudy: rain P.M. 3. Cloudy. 4—8. Fine. 9. Cloudy: rain A.M. and P.M. 10. Stormy: rain early A.M. 11. Cloudy: rain P.M. 12. Rain: rain early A.M.: rain A.M. 13. Cloudy. 14. Cloudy: rain and hail P.M. 15. Cloudy. 16—18. Fine. 19. Cloudy: rain early A.M. 20. Fine. 21. Cloudy. 22. Rain. 23. Fine. 24. Rain. 25. Cloudy. 26, 27. Fine. 28. Cloudy. 29. Fine. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—May 1. Beautiful summer day: heavy dew. 2. The same, but droughty. 3. Still fine, though getting cloudy. 4. Gentle rain all day: everything refreshed. 5. Moist A.M.: cleared up P.M. 6. Fine day: evening cool. 7. Hoar frost early A.M.: clear and calm. 8. Very warm: air electrical: cool P.M. 9. Dry and parching: very chill. 10. Withering day: wind piercing. 11. Wind changed to N.W.: returned to E. P.M. 12. Calm and warm: cool P.M. 13. Cloudy: very slight showers. 14. Frost: ice on the pools: slight snow showers. 15. Strong frost A.M.: getting cloudy: slight showers. 16. Temperature rising, but still cold. 17. Getting cloudy, but barometer still rising. 18. Fine soft rain nearly all day. 19. Warm and sunny throughout. 20. Beautiful summer day. 21. Very droughty, though one or two slight showers. 22. Withering day. 23. Boisterous weather and withering. 24. Temperature improving: cool P.M. 25. Quiet day: bright sunshine. 26. The same: rather showery P.M. 27. Droughty in the extreme. 28. Atmosphere highly electrical. 29—31. Not a cloud visible.

Sun shone 29 days. Rain fell 4 days. Frost 2 days. Snow 1 day.

Wind northerly 13 days. Southerly 11 days. Easterly 5 days. Westerly 2 days.

Calm weather 11 days. Brisk 6 days. Moderate 9 days. Boisterous 3 days. Strong steady breeze 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. May.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max.	Min.		9 a.m.	9 p.m.									
1.	29·986	29·992	29·891	29·45	29·94	29·92	56·0	61·2	51·8	73	35	61	52½	53	SW.	NE.	calm	S.	...	...	...	...	48
2.	29·948	29·967	29·916	29·44	29·99	29·98	60·4	64·2	51·6	72	43	57	54½	50	NW.	NE.	calm	ESE.	...	...	...	...	49
3.	29·976	29·974	29·889	29·50	29·92	29·89	53·6	66·3	45·8	70	43	50	55	50	NNW.	NW.	calm	SSW.	...	...	·03	...	51
4.	29·832	29·825	29·679	29·33	29·68	29·57	59·3	61·6	51·0	71	48	57	48	49	E.	S.	calm	S.	...	...	...	...	50
5.	29·686	29·838	29·669	29·05	29·62	29·90	57·4	66·7	51·9	71	44	62·5	56	48	S.	S.	calm	E.	...	...	...	...	52
☾ 6.	29·946	30·040	29·921	29·53	30·10	30·15	59·7	66·7	46·0	69	43	56	50	40	NE.	NE.	E.	NE.	...	...	...	0·30	51
7.	30·066	30·045	29·973	29·68	30·12	30·03	55·4	63·2	43·2	71	41	52·5	51½	55	NE.	E.	E.	S.	...	...	...	...	47
8.	29·894	29·896	29·843	29·53	29·98	29·98	57·9	61·3	43·7	72	44	53	58	48½	NE.	E.	E.	S.	...	·13	...	...	48
9.	29·872	29·865	29·819	29·53	30·07	30·13	47·2	67·4	46·3	53	39	47	40½	37	NNE.	NE.	NE.	NNE.	·338	·42	...	...	47
10.	29·864	30·026	29·871	29·53	30·19	30·26	45·4	51·9	41·0	55	42	43·5	46	40	N.	NE.	E.	NE.	·422	...	·14	...	43
11.	30·160	30·145	29·988	29·78	30·24	30·12	47·7	51·4	42·2	55	38	45	47	47½	N.	NE.	NE.	NW.	·033	...	...	...	42
12.	30·044	30·063	30·052	29·57	30·14	30·06	48·3	51·3	41·3	58	41	46	53½	50½	NW.	NE.	calm	NW.	...	·10	·12	...	42
● 13.	29·920	29·947	29·662	29·35	29·77	29·77	49·2	58·8	42·3	64	34	53	55½	36	NW.	W.	W.	NW.	·068	·13	·06	...	42
14.	29·548	29·574	29·490	29·15	29·67	29·49	39·2	50·7	36·2	52	29	43	45½	36½	NW.	N.	N.E	N.	·172	·01	...	0·00	42
15.	29·386	29·400	29·357	28·97	29·35	29·30	45·2	56·8	35·4	53	28	44·5	41½	39½	SE.	S.	calm	NW.	·016	...	·10	...	39
16.	29·556	29·816	29·550	29·05	29·33	29·62	46·4	63·4	33·7	58	28	47	45	43	S.	S.	NW.	W.	·166	...	...	...	38
17.	30·020	30·099	29·999	29·50	29·78	29·83	50·2	59·8	39·0	63	31	53	48½	48	S.	W.	calm	S.	...	...	...	...	35
18.	30·124	30·117	30·068	29·62	29·81	29·76	53·7	69·6	42·8	64	50	56	51	50	S.	SW.	W.	S.	...	·03	...	...	37
19.	30·066	30·173	30·066	29·43	29·94	30·10	53·7	54·3	52·2	70	50	56	55	51½	S.	SW.	calm	S.	·038	...	·09	...	42
☽ 20.	30·248	30·239	30·236	29·68	30·20	30·25	60·3	61·2	52·3	73	54	62	55½	48½	S.	E.	SE.	WNW.	...	...	...	...	48
21.	30·188	30·200	30·048	29·68	30·11	30·03	58·2	66·5	55·0	72	42	53·5	47	48½	NW.	NW.	calm	NW.	...	...	...	...	47
22.	30·004	30·169	29·994	29·46	30·15	30·20	50·5	65·3	47·4	55	38	45	49½	45	NW.	N.	N.	ENE.	...	...	...	0·40	49
23.	30·156	30·162	30·048	29·60	30·02	29·89	49·8	58·0	41·6	64	43	49	49	47	NW.	NW.	NE.	NW.	·022	...	·05	...	45
24.	29·946	30·067	29·946	29·36	30·03	30·10	50·3	51·7	45·2	64	36	45	51½	44	NW.	N.	NE.	NNW.	...	...	...	...	43
25.	30·120	30·202	30·096	29·65	30·13	30·12	47·7	52·0	41·7	55	29	48·5	51½	47½	N.	N.	N.	S.	...	...	·07	...	43
26.	30·216	30·226	30·196	29·70	30·13	30·13	48·3	60·3	40·7	66	33	53	50	49	NE.	NE.	calm	S.	...	...	...	...	42
27.	30·230	30·232	30·201	29·73	30·16	30·20	56·3	72·2	45·0	70	40	60	57	53	S.	NE.	calm	SW.	...	...	...	...	41
○ 28.	30·250	30·248	30·227	29·75	30·22	30·26	55·5	66·0	47·2	64	40	49·5	59	55	E.	E.	calm	NE.	...	...	...	...	43
29.	30·212	30·221	30·133	29·68	30·29	30·27	49·4	50·3	41·6	72	46	58	61	45½	NW.	NE.	E.	NE.	...	...	...	0·00	50
30.	30·090	30·095	30·055	29·62	30·25	30·24	53·8	54·5	48·0	72	52	53	52	50	N.	E.	E.	NE.	...	...	...	...	45
31.	30·008	30·010	29·985	29·58	30·19	30·14	56·3	70·2	52·2	68	49	52·5	51½	48	NW.	NE.	NE.	NE.	...	...	...	...	50
																			...	...	...	...	54
Mean.	29·986	30·028	29·928	29·50	29·98	29·99	52·3	60·5	45·0	64·48	40·42	52·	51·1	46·9					Sum. 1·275	·82	0·66	0·70	Mean. 45·3

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AUGUST 1839.

XVII. *On the Inaction of Amalgamated Zinc in acidulated Water.* *By* W. R. Grove, *Esq., M.A., M.R.I.**

IT is a well known fact, that the common zinc of commerce, when immersed in water acidulated by sulphuric, phosphoric, or muriatic acid, is rapidly dissolved, evolving torrents of hydrogen gas, while zinc with the surface amalgamated, remains inactive, under similar circumstances, unless touched by another metal placed in the same solution, in which case hydrogen, amounting to the full equivalent of the oxygen which unites with the zinc, is evolved from the surface of the associated metal, and the zinc is tranquilly dissolved.

M. De la Rive observed, that pure zinc is much less vigorously attacked by diluted acid than commercial zinc, and has thence, after a careful experimental investigation, concluded, that the evolution of gas by common zinc arises from the circumstance of its adulteration by other metals; thus an infinity of minute voltaic circles is established, the particles of zinc being oxidated, while those of the more negative metals evolve hydrogen (*Bib. Univ.* 1830). This explanation does not apply to the inactivity of amalgamated zinc, for as M. Becquerel asks (*Traité*, vol. v. p. 8), "Why does not mercury, which by its contact with zinc and acidulated water must also form a voltaic combination, produce a similar effect?"

An accidental circumstance led me to some experiments which I think give a satisfactory answer to this question; the

* Read before the Royal Academy of Sciences of Paris, on the 24th of June, 1839; and now communicated by the Author.

circumstance to which I allude, and which has in all probability been observed by many others, was this: in decomposing by a voltaic battery, water acidulated with sulphuric acid, there happened to be a few globules of mercury at the bottom of the operating cell, or glass containing the electrodes of platinum. I remarked that whenever the negative electrode touched the mercury it became amalgamated: at first I attributed this effect to the reduction of a film of oxide of mercury by the nascent hydrogen, but on touching the negative electrode thus amalgamated with the positive one, the latter also became frequently amalgamated. After several experiments I found that mercury which had acted in acidulated water as negative electrode of a voltaic battery, possessed the property of amalgamating platinum and iron, and that strips of platinum, iron, and even steel, which had served as negative electrodes, were instantly and perfectly amalgamated by immersion in pure mercury.

Having cleansed from acid particles several portions of mercury which had been used as negative electrodes, I found that they invariably gave an alkaline reaction, and it now became evident that the increased power of amalgamation proceeded from the mercury being alloyed with an alkaline metal. Remembering the highly electro-positive state of mercury which contains the slightest traces of an alkaline metal, a property first noticed by Sir Humphry Davy, it occurred to me that the inaction of amalgamated zinc was the effect of polarization*, but of one which differed from ordinary cases of polarization, in that the cations of the electrolyte instead of being precipitated on the surface of the negative metal, combine with it and render it so completely positive that the current is nullified, and not merely reduced in intensity as in other cases. To verify this idea I made the following experiments.

1. I amalgamated half the surface of a strip of copper, and immersed it, and a strip of zinc in water, acidulated with $\frac{1}{7}$th of sulphuric or phosphoric acid: on making the plates touch there was a rapid evolution of gas from the unamalgamated part of the copper, while only a few detached bubbles appeared on the amalgamated portion.

2. I placed a large globule of mercury (about half an ounce) in the bottom of a glass of acidulated water, and by means of a copper wire, the whole surface of which was amalgamated, made it communicate with one extremity of a galvanometer,

* I know of no other word to express the effect here alluded to; the word is used in this sense by most French writers, but, from its numerous applications, is sadly inaccurate.

while a strip of amalgamated zinc immersed in the same liquid communicated with the other extremity: at the instant of communication an energetic current was indicated, which however immediately diminished in intensity, and at the end of a few minutes the needle returned to zero; scarcely any gas was evolved, and of the few bubbles which appeared, as much could be detected on the surface of the zinc as of the mercury.

3. With the same arrangement I substituted for the mercury a strip of platinum well amalgamated. In this case, as before, after a few minutes the current became null, or so feeble as to require a delicate instrument to indicate its existence; and if, after the cessation of the current, the zinc was changed for unamalgamated platinum, this latter evolved torrents of hydrogen, and the needle indicated a violent current in a contrary direction.

4. With things arranged as in experiment 2. I employed a solution of sulphate of copper as an electrolyte instead of acidulated water: an energetic and constant current was produced, the mercury became amalgamated to saturation with reduced copper, and the precipitation of copper upon this amalgam continued as long as crystals of the sulphate were added to the solution.

By these experiments it appears that mercury, which in its normal state is well known to be inefficient as the positive metal of a voltaic combination, is in many cases equally inefficient as a negative metal, from its faculty of combining with the cations of the electrolytes, which, rendering it equally positive with the metal with which it is voltaically associated, the opposed forces neutralise each other. But if, as in experiment 4, the cation of the electrolyte is not of a highly electro-positive character; the zinc (or other associated metal) retains its superior oxidability and the voltaic current is not arrested. The application of these experiments to the phænomena presented by amalgamated zinc, is evident: all the heterogeneous metals with which the zinc may be adulterated and which form minute negative elements, being amalgamated, become by polarization equally positive with the particles of zinc, and consequently without the presence of another metal to complete the circuit, all action is arrested, as in the case of pure zinc. The fact of amalgamated zinc being positive with respect to common zinc, of its precipitating copper from its solutions, and other anomalies, are also explained by these experiments.

As, in a common voltaic combination of zinc and mercury this effect is complicated by the variety of cations which are transferred to the negative metal, for instance, hydrogen, zinc,

and alkaline matters*, I was anxious to discover whether hydrogen alone could, by its combination in small quantities with mercury, give it the same electro-positive qualities. Sir Humphry Davy, aided by a costly apparatus, scarcely succeeded in purifying water of alkaline matters, but the affinity of mercury for the alkaline metals gave me some hopes of attaining this object with less expensive means: to this end I submitted to electrolyzation for five days in a vessel of bees wax, distilled water acidulated with pure sulphuric acid; the negative electrode, of amalgamated copper, terminating in a mass of mercury. At the end of this period I removed the mercury, substituting a fresh portion of the same metal which was perfectly pure, and renewed the electrolyzation for two hours; I then quickly inclosed this last mercury in a tube with the purified water; it always evolved a small quantity of hydrogen, but I could not determine with certainty that its volume bore any given proportion to that of the mercury employed. Although this last portion of mercury when carefully cleaned and tested with reddened litmus paper gave no alkaline reaction, yet its existence might be suspected as derived from the wax; and as metallic vessels were obviously objectionable, I sought other means of determining the part played by the hydrogen in this combination. I repeated experiment 2, keeping heated, below the boiling point, the vessel containing the zinc and mercury; my galvanometer gave a tolerably constant deflection of 60 degrees, and the mercury evolved much more hydrogen than when the apparatus was cold. Again, I kept for some time a strip of well-cleaned platinum in hydrogen gas and then immersed it in mercury; when either platinum or mercury was moist I perceived a tendency to amalgamation, but none when they were perfectly dry. As I hope to renew this examination with a more perfect apparatus I will not detail any more of these experiments, but merely state my impression to be, that mercury under the influence of a voltaic current is capable of absorbing a very small quantity of hydrogen, which it gives up as soon as the communication is interrupted†.

* The current is not so completely null when dilute muriatic acid is the electrolyte, as it is with sulphuric or phosphoric acid; perhaps the sulphur or phosphorus contributes to the effect; the difference is however but trifling.

† The probability of a temporary combination of hydrogen with mercury throws some light upon the movements of mercury submitted, under an electrolyte, to a voltaic current: the hydruretted particles of mercury are repelled until out of the immediate influence of the current, where they yield their hydrogen, and so on of the rest. In electrolyzation with a mass of mercury as negative electrode, the hydrogen is all evolved at the parts most distant from the positive electrode.

In order to see whether this property of complete polarization was proper to mercury, or common to all metals in a state of fusion, I caused two currents, one proceeding from a voltaic pair consisting of zinc and a fused globule of Darcet's alloy, the other from zinc and mercury kept at the same temperature, to pass in contrary directions through the wire of a galvanometer; the current proceeding from the first pair was much more energetic than that from the second, and kept the needle constantly at 85 degrees. I could not repeat the experiment with other metals, from the impossibility of keeping them fused without volatilizing the electrolyte. As far as this case goes, it would seem that metals possess different polarizing capacities. I have before remarked a difference in the proportionate diminution of the current by polarization with solid metals, and think the subject merits an experimental examination: it is of some importance to the arts, as likely to lead to effectual means of preserving metals from oxidation.

July 1, 1839.

XVIII. *Notice of a lost Manuscript of the Seventh Book of the Mathematical Collections of Pappus Alexandrinus.* By J. O. Halliwell, *Esq., F.R.S., F.S.A., &c.**

IN the Advocates' Library at Edinburgh is preserved a very valuable and beautiful manuscript of five books of the Mathematical Collections of Pappus, viz. the 3rd, 4th, 5th, 6th, and 8th. To supply the deficiency of the seventh book, the most valuable one in the series, Dr. Moor, of Glasgow, procured a transcript of that part to be made from one of the Paris manuscripts by the celebrated Greek scholar Caperonier, at the commencement of the last century. This transcript was for some time in the hands of Dr. Robert Simson, who enriched it with MS. notes. In the dispersion of Dr. Moor's library the transcript was lost, and Dr. Trail in his Life of Simson regrets not having been able to discover its situation. About a year ago I accidentally found this identical MS. in a bundle of waste paper at a bookseller's shop in London, and, though unbound, quite perfect: my late lamented friend Professor Rigaud, who had it for some time, thus writes concerning it: "There cannot be the slightest doubt of its being Dr. Moor's transcript, for I have compared the hand-writing with Caperonier's other transcript in the Savilian Library, and find them agree. I congratulate you on having obtained a curious relic of Dr. Simson's favourite

* Communicated by the Author.

studies." These particulars may interest those readers of your Magazine who have ever paid any attention to the history of the ancient geometry.

XIX. *Note of an Analysis of Colophonite.* *By* Mr. T. Richardson.*

THE specimen subjected to analysis appeared remarkably pure, and was sent from Norway to my friend Mr. Hutton. It possessed the following characters:

It consisted of small round particles which could easily be detached from each other; spec. grav. 3·610; colour yellow, with a shade of brown; semitransparent; fracture uneven; lustre resinous.

20 grs. of the mineral analysed with every care in the usual way afforded the following result:

Silica	37·60	containing	19·54 oxygen.
Alumina	14·40	6·72	10·81
Perox. iron and manganese	13·35	4·09	
Magnesia	6·55	2·53	10·34
Lime	27·80	7·81	
Water	1·00		
	100·70		

which evidently corresponds with the formula

$$(Ca\,O\ Mg\,O)_3\ Si\,O_3 + (Al_2\,O_3\ Fe_2\,O_3)\ Si\,O_3$$

and also agrees with Trollé Wachtmeister's fundamental formula for the garnet, viz. $3\ Re,\ Si\,O_3 + R_2\,O_3\ Si\,O_3$; but differs from his analysis in containing alumina, which has replaced a certain quantity of peroxide of iron, hereby adding one more to the many already existing proofs of the isomorphism of these two bodies.

XX. *On Pyroxylic Spirit and its Compounds.* *By* Thomas Thomson, *M.D., F.R.S., &c. and Regius Professor of Chemistry in the University of Glasgow.*

[Continued from p. 51.]

1. *Sulphate of methylene.*

NO compound made from alcohol corresponding to this is known. The simplest method of obtaining it is to distil one part of pyroxylic spirit with eight or ten parts of concen-

* Communicated by the Author.

trated sulphuric acid. As soon as ebullition commences there passes into the receiver an oily liquid, mixed with a methylic liquor. This oily liquid becomes gradually very abundant, and when the distillation is finished, its quantity is at least equal to that of the pyroxylic spirit employed. The acid mixture should be distilled slowly, but the boiling should be constantly kept up.

This oily liquid being separated by decantation from the aqueous liquor, is first agitated with a little water, and then with a little chloride of calcium. It is then rectified several times successively over caustic barytes in a very fine powder. Finally, it is proper to leave it for some time in the vacuum of an air-pump with concentrated sulphuric acid and potash. The object of these processes is to separate some sulphuric acid, sulphurous acid, water, and pyroxylic spirit, which the liquid may contain.

When pure it is colourless, has an alliacious smell, and a specific gravity of 1·324 at the temperature of $71\frac{1}{2}°$. It boils at the temperature of 370°, under a pressure of thirty inches of mercury.

Dumas and Peligot subjected it to analysis by means of oxide of copper, and found 100 parts of it to yield

Carbon	19·03
Hydrogen	4·78

Now, these two numbers are to each other in the proportion of

2 atoms carbon	= 1·5
3 atoms hydrogen	= 0·375

If we consider it as composed of

2 atoms carbon	= 1·5	or per cent. 19·04
3 atoms hydrogen......	= 0·375	— — 4·76
1 atom oxygen	= 1·000	— — 12·70
1 atom sulphuric acid	= 5·000	— — 63·50
	7·875	100·00

Then it consists obviously of

$$C^2 H^3 O + S O^3$$

If $C^2 H^3 O$ be methylene, it is a sulphate of methylene.

Dumas and Peligot attempted to determine the density of the vapour of sulphate of methylene. They obtained as a result 4·565. But they do not consider this number as deserving of confidence. There can be little doubt that the true specific gravity of the vapour of this substance is 4·3750, for it obviously consists of

1 volume methylene vapour	1·5972
1 volume sulphuric acid	2·7777
	4·3750

The sulphate of methylene may not only be distilled over without alteration, but it may be heated to 392° without undergoing any decomposition. It is slowly decomposed by cold water, and rapidly by boiling water. The last acts with violence, evolving much heat, and the sulphate disappears altogether, without producing any new oil. Sulphomethylic acid is formed, and pyroxylic spirit is regenerated.

Caustic barytes has no action on it. Hydrate of barytes, and the hydrated alkalies, or their aqueous solutions, decompose it with great facility. Thus, solution of potash converts it, with much heat, into sulphomethylate of potash and pyroxylic spirit.

By means of sulphate of methylene all the other compounds of methylene and acids may be obtained. Thus, when heated with fused common salt, sulphate of potash is formed, and muriate of methylene is disengaged in the gaseous form. When heated with fluoride of potassium, hydrofluate of methylene is disengaged in the gaseous state. When heated with cyanodide of mercury, or of potassium, sulphate of potash, or sulphate of mercury is formed, and cyanodide of methylene may be collected in the liquid form. When it is distilled with benzoate of potash, we obtain benzoate of methylene; and so on.

When 2 parts of pyroxylic spirit, 2 parts of binoxide of manganese, and 3 parts of sulphuric acid, diluted with their own weight of water, are mixed, a violent effervescence takes place, and a great deal of formic acid is evolved. The other products from the distillation of this mixture have been examined with great care by Mr. Kane*.

2. *Nitrate of methylene.*—This compound was obtained by Dumas and Peligot by putting into a retort 50 parts of nitre, 100 parts of sulphuric acid, and 50 parts of pyroxylic spirit. The retort should be large, and connected with a large receiver communicating with a bottle containing salt water, and surrounded with a refrigerating mixture, and from this should pass a tube capable of conducting the gas formed into the chimney. It is only necessary to apply heat at the commencement of the process. Afterwards it goes on of its own accord. When the process is finished, the liquid in the receiver is poured into the bottle. In this way we obtain at the bottom of the bottle a colourless layer of the new compound. It must be separated by decantation and purified by distilling it off a mixture of massicot and chloride of calcium.

Thus prepared it is impure. If it be heated to the temperature of 140°, it boils and gives off a substance having a

* *Annalen der Pharmacie*, xix. 175.

decided odour of hydrocyanic acid. The temperature gradually rises to 151°. What comes over at that temperature is considered by Dumas and Peligot to be in as pure a state as they could procure it.

It is a colourless liquid, having a specific gravity of 1·182 at the temperature of 71°. It boils at 151°, giving out a weak ethereal odour. It is perfectly neutral, and burns with a lively yellow flame. When the vapour is heated to about 302°, it detonates with great violence, so as to produce dangerous results if the quantity be considerable.

Dumas and Peligot analyzed it by means of oxide of copper. The result of 5 analyses made in this way led to the conclusion that it was composed of

2 atoms carbon	= 1·5
3 atoms hydrogen	= 0·375
1 atom azote..............	= 1·75
6 atoms oxygen	= 6·00
	9·625

This is equivalent to $C^2 H^3 O + N O^5$.

They found the specific gravity of the vapour of nitrate of methylene to be 2·640. Now,

1 volume methylene........	= 1·5972
1 volume nitric acid vapour	= 3·7500
	2)5·3472
	2·6736

This result agrees as nearly as could be expected with the specific gravity of the vapour found.

When pyroxylic spirit is treated with nitric acid and silver, in the well-known method for obtaining detonating silver, no violent action takes place. Nitrate of methylene distils over; and towards the end of the process, if the nitric acid was strong, oxalate of silver is deposited*. The same thing happens when we substitute mercury for silver.

3. *Oxalate of methylene.*—The method of obtaining this compound, is to distil a mixture of equal parts sulphuric acid, oxalic acid, and pyroxylic spirit. There passes over into the receiver a spirituous liquor, which, when exposed to the air, speedily evaporates, leaving a residue crystallized in fine rhomboid plates. As the distillation proceeds, the quantity of this crystalline matter increases. At last the whole liquors that pass over assume a solid consistency. When the distillation is terminated, if we allow the retort to cool, and add

* Dumas and Peligot, *Ann. de Chim. et de Phys.* xi. 195.

as much pyroxylic spirit as at first, and distil a second time, we obtain the same product.

The crystals from these two distillations being well drained on a filter, are to be melted over an oil-bath to dry them, and distilled over massicot to free them from oxalic acid. The product thus obtained is pure oxalate of methylene.

It is colourless, and has a smell similar to oxalic æther. It melts at 124°, and boils at 322°, under a pressure of 30 inches of mercury. It dissolves in cold water, and speedily undergoes decomposition when thus dissolved, especially if it be heated, being converted into oxalic acid and pyroxylic spirit.

It is soluble in alcohol and pyroxylic spirit, and more soluble when these liquids are hot than when cold. The alkaline hydrates destroy it rapidly, forming oxalates and pyroxylic spirit. But anhydrous bases, or at least oxide of lead, do not alter it; anhydrous ammonia converts it into a new substance. Liquid ammonia converts it into oxamide.

Dumas and Peligot analyzed it by means of oxide of copper, and obtained for its constituents

Carbon	41·08
Hydrogen	5·28
Oxygen	53·64
	100·00

These numbers approach

4 atoms carbon	= 3	or per cent.	40·68
3 atoms hydrogen	= 0·375	— —	5·08
4 atoms oxygen	= 4	— —	54·24
	7·375		100·00

This is equivalent to $C^2 H^3 O + C^2 O^3$.

4. *Acetate of methylene.*—This compound may be obtained in abundance by distilling a mixture of 2 parts pyroxylic spirit, 1 part crystallizable acetic acid, and 1 part sulphuric acid of commerce. The product obtained is put in contact with a solution of chloride of calcium, which separates an abundant æthereal liquid, containing much acetate of methylene. As it still contains some sulphurous acid, and some pyroxylic spirit, it is agitated with quick-lime, and then left to digest over chloride of calcium for 24 hours, which absorbs the pyroxylic spirit. It is a colourless æthereal liquid, having an agreeable odour, analogous to that of acetic æther. It boils at $136\frac{1}{2}$° under a pressure of 30 inches of mercury. Its specific gravity is 0·919 at the temperature of $71\frac{1}{2}$°. Its constituents, determined by the analysis of Dumas and Peligot, are

Carbon	49·2	or 6 atoms	= 4·5	or per cent.	48·65
Hydrogen	8·3	or 6 atoms	= 0·75	— —	8·11
Oxygen	42·5	or 4 atoms	= 4·00	— —	43·24
	100·0		9·25		100·00

This is equivalent to $C^2 H^3 O + C^4 H^3 O^3$.

The specific gravity of the vapour of acetate of methylene is 2·563, as determined by Dumas and Peligot. Now, the specific gravity of

1 volume methylene vapour......	= 1·5972
1 volume acetic acid vapour......	= 3·5416
	2)5·1388
	2·5694

M. Laurent passed a current of chlorine gas slowly through acetate of methylene, and then distilled the liquid, leaving out the first portions which contained two oils. He obtained a colourless liquid, heavier than water, insoluble in water, but soluble in æther and alcohol. It boiled at 293°, and could be distilled over unaltered. Liquid potash attacked it, the liquid became brown, and a vapour was disengaged having a strong smell and a sweet taste. Perhaps, also, formate of potash was formed. The liquid was analyzed by Laurent, and found composed of

Carbon	20·25
Hydrogen	1·71
Chlorine	63·09
Oxygen	14·95
	100·00

From these numbers (which are only distant approximations) he deduces the following formula:—

6 atoms carbon	= 4·5	or per cent.	22·09
3 atoms hydrogen	= 0·375	— —	1·84
3 atoms chlorine	= 13·5	— —	66·25
2 atoms oxygen	= 2·0	— —	9·82
	20·375		

But it would be unsafe to draw deductions from this analysis*.

5. *Formate of methylene.*—Dumas and Peligot obtained this compound by distilling in a retort a mixture of about equal parts of sulphate of methylene and dry formate of soda. When the mixture is gently heated, the reaction commences,

* *Ann. de Chim. et de Phys.*, lxiii. 382.

and the temperature becomes speedily high enough to allow the process to go on without the further application of artificial heat. A very volatile liquid passes into the receiver, which must be kept cool. This liquid is formate of methylene nearly in a state of purity.

To make it quite pure, it should be distilled first over a fresh quantity of formate of soda, and afterward alone in a dry retort over a water-bath.

Pure formate of methylene thus obtained is very volatile, lighter than water and has an agreeable æthereal smell. It was analyzed by Dumas and Peligot by means of oxide of copper, and found to be composed of

Carbon	40·66	or 4 atoms = 3	or per cent.	40
Hydrogen	6·83	or 4 atoms = 0·5	— —	6·7
Oxygen	52·50	or 4 atoms = 4	— —	53·3
	100·00	7·5		100·0

Equivalent to $C^2 H^3 O + C^2 H O^3$.

The specific gravity of vapour of formate of methylene, as determined by Dumas and Peligot, is 2·084. Now

1 volume methylene vapour ...	= 1·5772
1 volume vapour of formic acid	= 2·5672
	2)4·1644
	2·0822*

6. *Benzoate of methylene.* — This compound may be obtained by distilling a mixture of 2 parts of benzoic acid, 2 parts of sulphuric acid, and 1 part of pyroxylic spirit, and mixing the liquid which passes over into the receiver with water. The æthereal portion separates. After washing it two or three times with water, let it be agitated with chloride of calcium, decanted off, and distilled over dry massicot. Finally, let it be boiled till its point of ebullition becomes fixed. It ought to be $388\frac{1}{2}°$.

Benzoate of methylene is oily, colourless, and has an agreeable balsamic odour. Its specific gravity is 1·10 at $62\frac{1}{2}°$. It is insoluble in water, but it dissolves readily in pyroxylic spirit, alcohol, and æther.

Its constituents, determined by the analysis of Dumas and Peligot, are

Carbon	71·4	or 16 atoms = 12	or per cent.	70·59
Hydrogen	6·2	or 8 atoms = 1	— —	5·88
Oxygen	22·4	or 4 atoms = 4	— —	23·53
	100·0	17		100·00

* *Ann. de Chim. et de Phys.*, lxiii. 48.

Now these atomic numbers may be grouped as follows:—

1 atom methylene	=	$C^2\ H^3\ O$
1 atom benzoic acid..............	=	$C^{14}\ H^5\ O^3$
		$C^{16}\ H^8\ O^4$

Thus its constitution is precisely similar to that of the other salts of methylene which have been already described.

Dumas and Peligot found that benzoate of methylene may be obtained by distilling a mixture of dry benzoate of soda and neutral sulphate of methylene.

The specific gravity of the vapour of benzoate of methylene, as determined by Dumas and Peligot, is 4·717. Now

1 volume methylene gas	=	1·5772
1 volume benzoic acid vapour	=	7·8475
		2)9·4247
		4·7123

7. *Mucate of methylene.*—This compound was first formed in 1836, by M. Malagutti*. The process for preparing it is precisely the same as that for preparing mucic æther, only substituting pyroxylic spirit for alcohol.

It is solid, crystallized, colourless, fixed, and insipid. It may be obtained in crystals, either from alcohol or water, but the crystals are not so distinctly shaped as those of mucic æther. When viewed with a microscope they appear to be rectangular prisms with beveled summits.

When heated, the mucate of methylene undergoes decomposition before it melts. Decomposition begins at the temperature of $325\frac{1}{2}°$, and shows itself by the evolution of a black oily matter; at 345°·2 it assumes the form of a black liquid, which swells and gives out carburetted hydrogen.

It is very little soluble in boiling alcohol, one part requiring 200 of alcohol of 0·814 to dissolve it. When the solution cools the mucate almost all falls under the form of a crystalline powder. It is very soluble in boiling water; but partly precipitates as the solution cools. The specific gravity of the crystals from alcohol is 1·48, that of those from water 1·53.

M. Malagutti analyzed this mucate of methylene, and obtained

Carbon	40·16	or 8 atoms = 6	or per cent.	40·34
Hydrogen	5·91	or 7 atoms = 0·875	— —	5·88
Oxygen	53·93	or 8 atoms = 8·0	— —	53·78
	100·00†	14·875		100·00

* *Ann. de Chim. et de Phys.*, lxiii. 94.

† *Ibid.* p. 295.

Equivalent to $C^2 H^3 O + C^6 H^4 O^7$. Thus it agrees in its composition with all the preceding compounds.

8. *Oxy-chloro-carbonate of methylene.*—When pyroxylic spirit is introduced to a glass vessel filled with chloro-carbonic acid the temperature rises suddenly, and the reaction is terminated in a very short time. Muriatic acid is formed, and chloro-carbonate of methylene, which separates under the form of a heavy oil when the pyroxylic spirit employed contains some water. It is easily separated from the water by decantation. It must then be rectified by distilling it by the vapour-bath over a great excess of chloride of calcium and massicot. Should it be suspected of still retaining any pyroxylic spirit, it may be digested without heat over fragments of chloride of calcium.

Thus purified it is a colourless liquid, very fluid, has a penetrating odour, is very volatile, and heavier than water. It burns with a green flame.

From the analysis of Dumas and Peligot, it follows that the constituents of this chloro-carbonate are

Carbon	25·57	or 4 atoms	= 3	or per cent.	25·26
Hydrogen	3·46	or 3 atoms	= 0·375	— —	3·16
Chlorine	37·12	or 1 atom	= 4·5	— —	37·90
Oxygen	33·85	or 4 atoms	= 4·0	— —	33·68
	100·00		11·875		100·00

Equivalent to $C^2 H^3 O + (C^2 O^3)$ Chl.

An atom of the water which enters into the constitution of pyroxylic spirit is decomposed, its hydrogen uniting to the chlorine of the chloro-carbonic acid, and converting it into muriatic acid, while the oxygen takes the place of that chlorine; so that 2 atoms of chloro-carbonic acid, $C^2 O^2 Chl^2$, become $C^2 O^3$ Chl, or an atom of oxychloro-carbonic acid*.

9. *Chlorocyanate of methylene.*—This compound was discovered in 1837 by M. Aimé, who obtained it by passing a current of chlorine through a solution of cyanodide of mercury in pyroxylic spirit†, and washing the liquid that distilled over in water.

Its specific gravity is 1·25. It boils at a heat under 122°. It burns with a red flame, green round the edges. Ammonia decomposes it immediately, and water in a few days. It is composed of

1 atom chloride of cyanogen ... C^2 Az Ch
1 atom methylene.................. $C^2 H^3 O$

10. *Cyanate of methylene.*—This compound was formed in

* *Ann. de Chim. et de Phys.*, lxiii. 52. † *Ibid.* lxiv. 222.

Liebig's laboratory, examined and analyzed by Mr. Richardson in 1837. It was obtained by passing liquid cyanic acid into pyroxylic spirit. The cyanate of methylene was deposited in the state of a white crystalline powder.

Its characters and composition are stated by Mr. Richardson to be the same as those of cyanic æther. It must then be $C^2 H^3 O + 2 (C^2 Az O) + 3 (H O)$.

M. Laurent has formed elaidate of methelyne, margarate of methylene, and oleate of methylene, by processes similar to what has already been described. For the characters of these compounds I refer the reader to Laurent's paper*.

Acid compounds of methylene.—The first of these formed by Dumas and Peligot they have distinguished by the name of *sulphomethylic acid.* It is obviously an acidulous salt, similar to *althionic* acid in its composition. This acid, together with tartromethylic and racemomethylic, the only ones hitherto examined, have been described in a preceding chapter of this volume.

Sulphamethylane.—When a current of dry ammoniacal gas is passed through pure sulphate of methylene, the liquid becomes very hot, and is gradually converted into a soft crystalline mass, which is probably a mixture of sulphate of methylene and *sulphamethylane.* To obtain this last compound it is merely necessary to treat sulphate of methylene with liquid ammonia. When the two liquids are agitated together a violent action takes place, and the sulphate of methylene disappears.

The liquid which remains after the reaction, being evaporated *in vacuo,* yields crystals of sulphamethylane, in large and beautiful plates. It is so deliquescent that it is difficult to preserve these crystals.

The analysis is difficult, but Dumas and Peligot think it probable that it is composed of

1 atom anhydrous sulphate of ammonia
1 atom anhydrous sulphate of methylene.

Oxamethylane. When a current of ammoniacal gas is passed through oxalate of methylene, a slight heat is evolved, but to produce the proper reaction, the oxalate of methylene must be kept in a state of fusion. The liquid gradually becomes solid, and becomes at last a white crystalline matter. When dissolved in boiling alcohol and allowed to cool, it crystallizes in cubes with pearly faces.

The constituents of this substance, according to the analysis of Dumas and Peligot, are

* *Ann. de Chem. et de Phys.,* lxv. 296.

Carbon	34·47
Hydrogen	5·06
Azote	13·90
Oxygen	46·57—100·00.

These proportions give the following atomic constituents :—

6 atoms carbon	= 4·5	or per cent.	34·95
5 atoms hydrogen	= 0·625	— —	4·85
1 atom azote	= 1·75	— —	13·59
6 atoms oxygen	= 6·00	— —	46·61
	12·875		100

These numbers are resolvable into

1 atom oxalic acid............	$C^2\ O^3$
1 atom methylene	$C^2\ O\quad H^3$
forming 1 atom oxalate of methylene	$C^4\ O^4\quad H^3$
1 atom oxamide...............	$C^2\ O^2 + H^2\ Az$
	$C^6\ O^6\quad H^5\ Az$

Thus we see that the oxamethylane is a compound of

1 atom oxalate of methylane......	7·375
1 atom oxamide	5·5
	21·875*

[In reference to note ‡ p. 42 of our last number, and to complete the history of the subject, we extract the following from the paper of Dumas and Peligot.

" *Note historique sur l'esprit de bois.*—Pour éviter toute confusion dans l'esprit du lecteur, nous n'avons voulu, dans le cours de ce mémoire, ni rapporter, ni discuter les idées et les analyses, qu'on a émises ou publiées sur l'esprit de bois. Nous donnerons ici un sommaire des recherches dont ce corps a été l'objet.

" La découverte de l'esprit de bois est due à M. Philip Taylor, dont le séjour prolongé en France nous a permis d'apprécier les rares talens. Il trouva l'esprit de bois en 1812, mais il ne publia ses observations à ce sujet qu'en 1822, et encore d'une manière occasionelle, dans une lettre adressée aux rédacteurs du *Philosophical Journal.*

" M. Dœbereiner venait d'annoncer qu'il avait trouvé de véritable alcool dans les produits de la distillation du bois; M. Taylor observe que le fluide particulier désigné comme alcool lui ressemble en effet sous beaucoup de rapports, mais qu'il en diffère par des qualités essentielles : comme lui il est volatil, inflammable et miscible à l'eau; comme lui il dissout le camphre et les gommes résinés: il peut le remplacer dans quelques applications industrielles, mais soumis à l'action de l'acide sulfurique concentré, il ne fournit point d'éther sulfurique. M. Taylor le regarde donc comme un produit particulier qu'il désigne sous le nom d'éther pyroligneux.

" Les observations de M. Philip Taylor sont de la plus parfaite exactitude, et nous avons reproduit sans peine l'esprit de bois avec toutes les qualités qu'il lui assigne."—*Annales de Chim. et de Phy.*, tom. lviii. p. 70.—Edit.]

* *Ann. de Chim. et de Phys.*, lxi. 60.

XXI. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 20.]

City of Varinas, Plains of Venezuela. Lat. 7° 40′ *N.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821.				
May 7.	76°	9 a.m.	Elevation about 500 feet.	
,,	81	4 p.m.		
8.	78	8 a.m.		Rain.
,,	86	1 p.m.		Fair.
9.	80	12		Rain.
10.	87	2½ p.m.		Fine.
11 & 12.	id.	id.		Id.
13.	86	10 a.m.		Id.
14.	80	2 p.m.		Cloudy.
16.	86	10½ a.m.		Id.
17.	90	2½ p.m.		Fine.
18.	85	9½ a.m.		Rain at night.
20.	85	2 p.m.		Fine.
21.	76	7½ a.m.		Rain.
22.	75	6½ a.m. }		
,,	85	2 p.m. }		Fine.
15 days. 85°·45 max. } 76 ·25 min. } Medium 80°·85.				

City of San Carlos, Plains. Lat. 9° 20′ *N.*

June 5.	80	5½ a.m.	Elevation 543 feet according to the latest barometrical measurements of Messrs. Rivero and Boussingault.	
,,	82	3 p.m.		Rain.
6.	79	6 a.m.		
,,	82	2 p.m.		Do.
7.	79	6 a.m.		
,,	83	2 p.m.		Fair.
8.	80	7 a.m.		Do.
,,	84	2 p.m.		
9.	80	7 a.m.		Do.
,,	85	2 p.m.		
10.	id.	id.		Do.
11.	85·5	4 p.m.		Do.
12.	79	8 a.m.		Rain at night.
14.	80	id.		Fine.
,,	84	2 p.m.		
15.	id.	id.		Do.
16.	79	8 a.m.		

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821. June 16.	82°	2 p.m.		Fine.
17.	78	8 a.m.		Rain at night.
18.	78	8 a.m.		
,,	82	2 p.m.		Fine.
13 days 83°·11 max. 79°·2 min. } 81°·15 med.				

City of Valencia, near the Lake of that name. Lat. 10° 9′ *N.*

June 29.	78	7 a.m.	Elevation	Fair.
,,	80	2 p.m.	of the	
30.	id.	id.	lake 1495	
July 1.	76	7 a.m. }	feet ac-	
,,	80	2 p.m. }	cording	Showery.
2.	id.	id.	to Hum-	Id.
3 and 4.	83	2 p.m.	boldt,	
5.	76	6½ a.m. }	1577 feet	
,,	82	4 p.m. }	accord-	Id.
6.	76	7 a.m. }	ing to	Id.
,,	82·5	4 p.m. }	Boussin-	
7.	76	6 a.m. }	gault.	Fair.
,,	82·5	4 p.m. }	The city	
8.	76	6 a.m. }	rather	Showery.
,,	83	4 p.m. }	higher.	
9.	75	6 a.m. }		Fair.
,,	83	2 p.m. }		
10.	76	6½ a.m. }		Id.
,,	85	4 p.m. }		
11.	80	6 p.m.		Rain.
12.	73	6 a.m }		Showers.
,,	81	2 p.m. }		
13.	74	6 a.m. }		
,,	82	2 p.m. }		
14.	75	6 a.m. }		Cloudy.
,,	82	2 p.m. }		
15 }	75	6½ a.m. }		Fair.
to 20. }	82	4 p.m. }		
21.	72	7 a.m. }		Rain.
,,	82	4 p.m. }		
22.	73	6 a.m. }		Fair.
,,	81	2 p.m. }		
23.	76	6 a.m. }		Cloudy.
,,	81	2 p.m. }		
24.	77	7 a.m.		
25.	81·5	2 p.m.		Fair.
26.	72	6 a.m. }		
,,	80	2 p.m. }		

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821. July 27.	77°	8 a.m.		Fair.
28.	76	6½ a.m.		Id.
,,	83	2 p.m.		
29.	76	6½ a.m.		Rain.
,,	80	2 p.m.		
30.	76	7½ a.m.		Fair.
,,	81	2 p.m.		Showers.
31.	76	7½ a.m.		Showers.
,,	80	2 p.m.		
33 days 81°·21 max. 75°·99 min.	78°·6 med.			
Aug. 1.	73	7 a.m.		
2.	78	2 p.m.		Rain.
3.	78	2 p.m.		
4.	76	7 a.m.		Shower.
,,	80	2 p.m.		
5. 6.	78	9 a.m.		Fair.
and 7.	80	2 p.m.		
8.	79	8 a.m.		Id.
,,	80	9 p.m.		
9. and 10.	75	7 a.m.		Id.
,,	82	2 p.m.		
10 days. 80° max. 75°·8 min.	Med. 77°·9, of 43 days, med. 78°·25.			

Town of Maracay, valleys of Aragua between Valencia and Caracas.

Aug. 15.	73	6½ a.m.	Level of the lake or 1577 feet.	
,,	84	12		Fine.
16.	73	6 a.m.		
,,	81	12		Id.
17.	75	6 a.m.		Showers.
18.	73	6 a.m.		Fine.
,,	82·5	4 p.m.		
4 days 82°·5 max. 73 ·5 min.	78° med.			

Town of La Vittoria, Valleys of Aragua.

Aug. 24.	84	12	1620 feet.	Fair.
25.	71	6 a.m.		
to 28.	84	12		Id.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821. Aug. 30.	87°	3 p.m.		
31.	70	7 a.m.		
„	85·5	4 p.m.		Fair.
7 days 85·5 max. 70·5 min. } 78° med. of the valleys of Aragua.				

City of Caracas. *Lat.* 10° 31′ *N.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
Sept. 8.	73	6 a.m.	2903 feet. The "*Silla*," or Saddle mountain above the city 8636 feet, nearly the same height as the city of Bogotà, and 878 feet below that of Quito.	
„	78	4 p.m.		Fine.
9.	73	6 a.m.		
„	79	3 p.m.		Fine.
10.	73	7 a.m.		
„	77	2 p.m.		Showers.
12.	74	6 a.m.		
„	78	3 p.m.		Fair.
13.	76	3 p.m.		Showery.
14.	72	6½ a.m.		Id.
„	75	3 p.m.		
15.	70	6½ a.m.		Id.
16.	67	7 a.m.		
„	75	3 p.m.		
18.	68	7 a.m. }		Fine.
„	76	2 p.m. }		
19.	69	6½ a.m.		Showery.
20. 21. 22.	} 75	2 p.m.		Fine.
23.	74	8 a.m. }		Id.
„	75	2 p.m. }		
24.	68	7 a.m. }		Id.
„	75	2 p.m. }		
25.	72	7 a.m. }		Id.
„	76	3 p.m. }		
26.	78	3 p.m.		Id.
27.	70	7 a.m.		Id.
„	78	3 p.m.		Id.
28.	68	7 a.m.		
29 & 30.	78	3 p.m.		Id.
21 days 75°·2 max. 69·71 min. } 72°·45 med.				
Oct. 1, and 2.	} 70	7 a.m.		Id.
„	75	2 p.m.		
3, 4, and 5.	} 76	12		Id.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821.				
Oct. 6.	79°	2 p.m.		Fine.
7.	69	7 a.m.		Id.
8.	76	12		
9.	70	7 a.m.		
10.	77	3 p.m.		
11.	75	3 p.m.		Showers.
12.	70	6 a.m.		
13, and 14.	78	3 p.m.		Fine.
15.	70	6 a.m.		
,,	79	1 p.m.		
16 & 17.	70	6 a.m.		Id.
,,	77	1 p.m.		
18, 19,	70	8 a.m.		
and 20.	79	2 p.m.		Id.
21.	69	7 a.m.		
22.	78	2 p.m.		
23.	70	7 a.m.		Id.
,,	79	3 p.m.		
24.	80	12		
25, 26, and 27.	70	7 a.m.		Showery.
	75	3 p.m.		
28 to 31.	69	7 a.m.		Fine.
	75	3 p.m.		
31 days 77°·62 max. 69° min. } 73° 31′ med.				
Nov. 1.	69	7 a.m.		Showers.
,,	75	3 p.m.		
2.	73	3 p.m.		Id.
3.	74	3 p.m.		Id.
4, 5 and 6.	75	3 p.m.		Fair.
	,,	,,		
7.	69	7 a.m.		Id.
,,	78	3 p.m.		Id.
8.	76	2 p.m.		Id.
9.	69	7 a.m.		Id.
10and11.	69	7 a.m.		Id.
12 to 18.	69	7 a.m.		Fair.
	77	2 p.m.		Id.
19.	78	2 p.m.		
20.	69	7 a.m.		Id.
21.	67	7 a.m.		Id.
22.	78	2 p.m.		Id.
23.	67	7 a.m.		Id.
24.	76	2 p.m.		Id.
25.	67	7 a.m.		Id.
26, 27	66	7 a.m.		Id.
28.	75	3 p.m.		Id.

Table *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1821.	°			
Nov. 29.	66	7 a.m.		Fair.
30.	74	3 p.m.		Id.
30 days 76°·71 max. 68 ·37 min. } 72°·54 med.				
Dec. 1.	66	7 a.m.		Fair.
2.	74	3 p.m.		
3 to 8.	} 66	7 a.m.		
	} 76	3 p.m.		
9.	71	10 a.m.		
,,	76	3 p.m.		
10 to 20.	} 65	7 a.m.		Foggy mornings, with light showers, in the mornings.
	} 75	3 p.m.		
21.	71	5 p.m.		Fair.
,,	75	3 p.m.		
22	70	8 a.m.		Id. mornings cloudy.
to 31.	75	3 p.m.		
31 days 75°·4 max. 68 ·0 min. } 71°·7 med.				
1822.				
Jan. 7	66	7 a.m.		Fair.
to 14.	70	3 p.m.		
15.	71	3 p.m.		Do.
16. to 24.	} 66	7 a.m.		Do.
	} 70	3 p.m.		Do.
25.	75	3 p.m.		Do.
26.	65	8 a.m.		Do.
27.	72	3 p.m.		Do.
28.	68	8 a.m.		Do.
,,	73	3 p.m.		
29.	61·5	7 a.m.		Do.
,,	71	3 p.m.		
30.	65	7 a.m.		Do.
31.	71	3 p.m.		Do.
24 days 71°·36 max. 65 ·1 min. } 69·73 med.				
February. 28 days.	70°·19 max. 65°·85 min. } 68·02 med.			Weather fine with few showers.
March. 31 days.	73°·31 max. 68°·7 min. } 71° med.			Weather fine throg hout the month.
April. 30 days.	71° med.			Fair, cloudy, and showers towards the end.

TABLE *continued.*

May. 31 days.	73°·88 max. 69·47 min. } 71° 67′ med.	Cloudy and showery.
June. 30 days.	74°·56 max. 69°·93 min. } 72° 24′ med.	Fair with showers.
287 days.	Average temperature 71° 40′.	

Venezuelan Chain of the Andes, from Valencia to the Valleys of Cucuta.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1822. July 29.	72°	7 a.m.	485 ft.	Barquisimeto. Lat. 8° 55′ N. Weather fair and wind.
„	78	3 p.m.		
30.	72	7 a.m.		Id.
„	78	3 p.m.		
Aug. 2.	77	6½ a.m.	2058 ft.	Tocuyo. Lat. 9° 35′ N. Weather fine.
„	81	2 p.m.		
8.	65	6 a.m.		
„	74	4 p.m.		Carache. Weather fine.
11.	72	6 a.m.	2684 ft.	Truxillo. Lat. 8° 40′ N. Weather fine.
„	81	3 p.m.		
12.	77	6 a.m.		Valera.
14.	67	9 a.m.		Timotes.
16.	53	7 a.m.		Macuchies.
19.	64	7 a.m.	5280	Merida. Lat. 8° 10′ N. Misty showers in the evening.
„	75	1 p.m.		
„	69	5 p.m.		
24.	58	6½ a.m.		La Grita. Weather fair.
27.	81	7½ a.m.		Capache.
28.	San Josè de Cucuta.			

General mean 72° 44′.

Chain between Cucuta and Bogotà.

Date.	Time.	Thermo-meter.	Elevation.	Remarks.
1822. Sep. 1. „	° 60 62	 8½ a.m. 2 p.m.	Elevation between 8000 and 10,000 feet.	Pamplona. Lat. 6° 30′ N. Rain.
3. „	54 59	8 a.m. 4½ p.m.		Chitaya. Fair.
5.	59	7½ a.m.		La Concepcion. Fair.
6.	75	8 a.m.	Lowest points of the valley, probably from 3000 to 5000 ft.	Capitanejo. Fair.
7. „ 8.	70 76 67	9 a.m. 2 p.m. 7 a.m.		Suata. Fair.
11. „	56 60	6½ a.m. 4 p.m.	Ascent.	Santa Rosa. Fair.
14. „	58 55	7 a.m. 4 p.m.		Tunja. Lat. 5° 5′ N. Fair.
18.	58	6½ a.m.		Enemocon. Fair.
19.	56	7 a.m.		Lipaquera. Rain.

General mean 61° 66′, or deducting the close valley of Capitanejo, as forming an inconsiderable portion of the country, 57° 91′.

City of Bogotà. Lat. 4° 6′ *N.*

Sept. 23.	60	7 a.m.	8694 feet.	Fair, partially cloudy.
24 & 25.	62	2 p.m.		Id.
26.	57	7 a.m.		Id.
to 29.	60	2 p.m.		Id.
30.	id.	id.		Id.
Oct. 1.	57	7 a.m.		Showery.
2 and 3.	62	2 p.m.		
4.	57	7 a.m.		Id.
5.	57	7 a.m.		Id.
to 23.	62	2 p.m.		Id.

30 days 61°·8 max. / 57·1 min. } 59° 45′ med.

[To be continued.]

XXII. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions.* By JAMES IVORY, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from p. 12.]

Atmosphere of dry air.

IN applying the formula (A.) to the experimental ascents that have been made in the atmosphere, σ may be accounted equal to z, the height ascended: for $\frac{z}{a}$, which is a minute fraction at the top of the atmosphere, is insensible in small elevations. Further, in such experiments, the depression of the thermometer, or the difference of the temperature at the upper and lower extremities of the ascent, is only a moderate number of degrees; and as β is a very small fraction, the value of q in the formula

$$q = \frac{\beta(\tau' - \tau)}{1 + \beta\tau'},$$

will be so inconsiderable, that its powers may be neglected. Attending to what is said, the formula (A.), even in those cases where the ascents are most considerable, may take this very simple form without much error, or rather with all the accuracy warranted by the nature of such experiments, viz.

$$z = \frac{p'}{\rho'} \cdot \frac{1+f}{f} \cdot \frac{\beta(\tau' - \tau)}{1 + \beta\tau'};$$

or, by making $D = \rho'(1 + \beta\tau')$,

$$z = \frac{1+f}{f} \cdot (\tau' - \tau) \cdot \beta \cdot \frac{p'}{D}.$$

Now it is obvious that D is the density of the air at the earth's surface, reduced to zero of the thermometer; and hence we learn that $\frac{p'}{D}$ is independent on the magnitude of p', and has the same value in all atmospheres of dry air; for, D being the density of the air produced by the pressure p' at the fixed temperature zero of the thermometer, it will vary proportionally to p'.

The value of the constant quantity $\frac{p'}{D}$ is next to be found. It has been ascertained, by very careful experiments, that the density of mercury is to the density of dry air as 10462 to 1, the temperature being 0° centigrade, or 32° of Fahrenheit's scale, and the barometric pressure $0^{m}\cdot76$, or 29·9218 English inches. The temperature remaining at 32° Fahrenheit, if

the barometric pressure be changed to p', the density of mercury will be to the density of dry air, at the temperature 32° Fahrenheit and under the pressure p', as $10462 \times \frac{29{\cdot}9218}{p'}$ to 1; wherefore, as D stands for the density of dry air in the circumstances mentioned, its value estimated in parts of the density of mercury, will be thus expressed:

$$D = \frac{1}{10462} \times \frac{p'}{29{\cdot}9218}:$$

hence

$$\frac{p'}{D} = 10462 \times 29{\cdot}9218;$$

and, by reducing the inches to fathoms,

$$\frac{p'}{D} = L = 4347{\cdot}8 \text{ fath.}$$

This quantity being found, we deduce from the foregoing formula for z,

$$\frac{1+f}{f} = \frac{1}{\beta L} \times \frac{z}{\tau' - \tau}.$$

A single experiment in which z and $\tau' - \tau$ were ascertained, should be sufficient for determining $\frac{1+f}{f}$ and f: but it is well known that great irregularity prevails in the rate at which the heat decreases in the atmosphere, more especially when the elevations are small. This is owing chiefly to the thermometer, which is often affected by local and temporary causes. When we reflect that a considerable variation in the height is required to produce a small change of the thermometer, even the errors unavoidable in the use of that instrument must produce notable discrepancies in the rate, when the whole observed difference of temperature is only a few degrees. It thus appears that the quantity sought cannot be determined with tolerable exactness, except by taking a mean of the results obtained from many experiments. In this view, the average estimations of the decrease of heat in the atmosphere, which have been inferred from their own researches by philosophers on whose judgement and accuracy dependence can be had, becomes very valuable. Professor Playfair, in his Outlines, states that the decrease of heat is nearly uniform for the greatest heights we can reach; and that it may be taken on an average as equal to 1° of Fahrenheit's thermometer for 270 feet, or 45 fathoms, of perpendicular ascent.

The same rate has the authority of Professor Leslie, to whom meteorology is so much indebted. If we make $z = 45$ fathoms, $\tau' - \tau = 1^\circ$, $\beta = \frac{1}{480}$, we shall obtain

$$\frac{1+f}{f} = \frac{45}{9{\cdot}05} = 5,\ f = \frac{1}{4},$$

which are the numbers assumed in the paper of 1823.

According to Dr. Dalton, another eminent philosopher who has studied meteorology very successfully, and made many experiments with great care, the average ascent for depressing Fahrenheit's thermometer 1° is 300 feet, or 50 fathoms; this gives

$$\frac{1+f}{f} = \frac{50}{9{\cdot}05} = 5{\cdot}5.$$

Ramond, in his Treatise on the Barometrical Formula, has recorded the heights for depressing the centigrade thermometer 1°, in 42 different experiments. Setting aside four of this great number on account of their excessive irregularity, he states the mean of the remaining 38 at $164^{\mathrm{m}}{\cdot}7$. A good average may be expected from so many experiments, made by observers of the greatest eminence, in different quarters of the world, in every variety of height and temperature. Now 4347·8 fathoms $= 7951^{\mathrm{m}}$; $\beta = \frac{3}{800}$; $z = 164{\cdot}7$; consequently

$$\frac{1-f}{f} = \frac{800 \times 164{\cdot}7}{7951 \times 3} = 5{\cdot}5.$$

It would be a great omission in this research to leave out the celebrated ascent of Gay-Lussac in a balloon. According to Laplace, the whole height ascended, or z, is 6980^{m}, the depression of the thermometer, or $\tau' - \tau$, being $40^\circ{\cdot}25$ centigrade: hence

$$\frac{1+f}{f} = \frac{800}{7951 \times 3} \times \frac{6980}{40{\cdot}25} = 5{\cdot}8.$$

It is to be observed that, although experience and theory both concur in proving that z and $\tau' - \tau$ increase together in the same proportion to considerable elevations in the atmosphere, yet, at very great elevations, there is no doubt that z increases in a greater ratio than $\tau' - \tau$: so that when very great heights are used for computing $\frac{1+f}{f}$, the resulting value will be greater than the true quantity. What is said

accounts sufficiently for the excess of $\frac{1+f}{f}$ deduced from Gay-Lussac's ascent, above the other values found from moderate elevations. Without further research we may adopt the following determinations as near approximations derived from a multitude of experiments,

$$\frac{1+f}{f} = 5{\cdot}5\,;\; f = \frac{2}{9}.$$

The difference of these numbers from those used in the paper of 1823, produces an increase in the refractions amounting to 19″ at the horizon, and to 2″ at 2° of altitude.

The irregular manner in which the heat decreases in such experiments as have been used for finding f, evidently makes them altogether unfit for determining the next coefficient f'. One remark respecting this quantity deserves to be noticed. By expanding the formula (4.), we obtain,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + f.\frac{u^2}{1.2} - f.\frac{u^3}{1.2.3} + \&c.:$$

the exact value is

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + (f-f').\frac{u^2}{1.2} - \&c.:$$

now, as these values continue very nearly equal to considerable elevations, the first terms of the two series must nearly coincide: which requires that f' shall be only a small part of f.

We have next to attend to the formula (B). As q is only a small fraction in all the elevations that have been reached in the atmosphere, its square and other powers may be neglected: so that,

$$\sigma = \frac{z}{1+\frac{z}{a}} = \frac{p'}{\rho'}\left(1-\frac{q}{2}\right)\log\left(\frac{p'}{p}\right):$$

and, because

$$1 - \frac{q}{2} = \frac{1+\beta\left(\frac{\tau'+\tau}{2}\right)}{1+\beta\tau'}\,;\frac{p}{\varrho'(1+\beta\tau')} = \mathrm{L}:$$

$$z = \left(1+\frac{z}{a}\right).\mathrm{L}\left(1+\beta\left(\frac{\tau'+\tau}{2}\right)\right)\cdot\log\left(\frac{p'}{p}\right).$$

Now this is nothing more than the usual barometric formula for measuring heights, as it is found in the writings of Laplace or Poisson. It supposes that unit represents the force of gravity at the earth's surface; and if the variable intensity

of that force in different latitudes must be taken into account, nothing more is requisite than to multiply by the proper factor. When this is done the foregoing expression will be identical with the usual formula, all its minutest corrections included. But there is this difference between the two cases, that the usual formula is investigated on the arbitrary supposition that the temperature is constant at all the points of an elevation, and equal to the mean of the temperatures at the two extremities; whereas the other expression is strictly deduced from the general properties of an atmosphere in equilibrium. The exact theoretical formula has been made to coincide with the approximate one, by dismissing all the terms that cannot be estimated in the present state of our knowledge of the phænomena of the atmosphere.

All the properties of the atmosphere that have been ascertained with any degree of certainty, have been made known to us by the application of the barometric formula: it would therefore be superfluous to attempt, by the consideration of particular experiments, any further elucidation of a theory which is, in a manner, identical with observation, as far as our knowledge extends.

[To be continued.]

XXIII. *On the Older Stratified Rocks of North Devon, with correlative Remarks concerning Transition or Protozoic Regions in general*.* *By* Thomas Weaver, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c. Communicated by the Author.*

In the more recent views set forth by Professor Sedgwick and Mr. Murchison on the *Classification of the older stra-*

* Objections have been repeatedly taken to the use of the term *transition series or system,* but I think without sufficient reason; for it is not just to argue from the abuse against the use of anything. To its original full and legitimate import I adverted upon a former occasion (in a note to the 8th § of my Memoir in vol. i. second series of Geol. Trans.), as denoting all fossiliferous rocks of an origin antecedent to that of the Old Red Sandstone. Being in this its pristine sense a defined collective term, it is as such highly useful, whatever subdivisions it may be thought right, now or hereafter, to adopt, whether under the denomination of Silurian, Cambrian, or other geographical designations. Wherever these subordinate distinctions may be found strictly applicable in other countries, there they may be appropriately employed, for the sake of greater clearness and precision; but in all cases of doubt affecting any portion of the series, and as a term of comprehensive meaning, embracing all its members, the expression *transition system* might yet retain its place in geological language; unless geologists in general should prefer the term *protozoic* or *palæozoic,* the former as lately proposed by Mr. Murchison, and the latter by Prof. Sedgwick. The chief objection to the term *transition,* as it appears to me, is, that it

*tified Rocks of Devonshire and Cornwall**, my Memoir on North Devon† is adverted to, and some allusion is also made to Ireland. I have much regretted that the state of my health has for a considerable time past prevented my attending the meetings of the Geological Society, or any one of those of the British Association. It would otherwise have afforded me greater satisfaction to have given any explanation that might have been required *viva voce*, rather than to have recourse to writing that which may now be deemed necessary.

With respect to the relative age of the culmiferous rocks of Devon, extending from the siliceous or argillaceous shales and carbonaceous limestone upward, it appears now to be generally admitted that they belong to the carboniferous series. But it is contended that from those siliceous and argillaceous shales downward there is both a conformity with, and a gradation into the subjacent Trilobite clayslates, which constitute the northern boundary of the carboniferous trough; and that this body of clayslate (according to the Rev. D. Williams three to five miles wide) with subordinate layers of limestone, together with the other old stratified rocks, (composed, in a descending order, of the Wollacomb and Marwood sandstones, Morte clayslates, Ilfracombe, Berry-narbor and Comb Martin limestones, Trentishoe sandstones and clayslates with quartzy clayslates and quartz-rock, Linton clayslates with beds of quartz-rock and sandstone and layers of limestone, and the Foreland sandstone), are to be considered as *the equivalents of the Old Red Sandstone*, and not referable partly to the Silurian and partly to the Cambrian system, as formerly stated ‡. As I do not concur in these new views, it

is a word of common use, applicable in many senses. On the other hand, the term *protozoic*, as signifying a class of rocks which entombs the remains of the earliest of created beings, being of a more confined and definite character, may be well entitled to our acceptation.

* Lond. and Edinb. Phil. Mag. April 1839, and Proceedings of the Geological Society, 24th April 1839.

† Proceedings of the Geol. Society, Jan. 1838, and L. & E. Phil. Mag. July 1838, Supplement.

‡ In the views previously entertained by the authors, the older stratified rocks of North Devon were distributed into five mineral masses, of which the four lower were referred to the upper part of the Cambrian system, and the fifth, or uppermost, to the lower part of the Silurian; while of the highest group of all, in the order of succession, the culmiferous series, it was shown that they *cannot form a true passage into the different schistose rocks upon which they rest.* (Proceedings of the Geological Society, June 1837, and in particular pp. 560 to 562.) See also the masterly views developed by Prof. Sedgwick (*Ibid.* May 1838, pp. 675, 676, *et seq.*).

The authors now refer the Older Stratified Rocks of Devon to a position intermediate between the Silurian and Carboniferous systems. (Lond. and Edinb. Phil. Mag., vol. xiv. pp. 248, 259. April 1839.)

appears to me but right to endeavour briefly to give some further explanation of the grounds upon which I formed my own individual opinion, as expressed in the Memoir read before the Geological Society, referred to above, and to which I still adhere; namely, that the Older Stratified Rocks of North Devon, though distinguished by some peculiarities, belong to the transition epoch. These additional reasons will also make manifest the principles upon which I differ from the new position taken by Professor Sedgwick and Mr. Murchison.

And first, as to *gradation*. When two formations are in juxta-position, gradation from one to the other may be conceived as taking place in two ways; either by an alternation between the beds of the two formations on the line of their conjunction; or, where there is no such alternation, by an incorporation and interchange of their individual characters, as dependent on mineral composition and the remains of the organized bodies which they contain respectively. Now, I have not found either of these cases applicable in North Devon to the line of junction of the Trilobite slates with the carboniferous strata. On the contrary, we pass at once from the Trilobite clayslates (partly glossy and finely laminated) and containing other peculiar organic remains, to coarse shales free from any such remains, and more or less indurated and siliceous, in connexion with the carboniferous limestone.

Secondly, as to *conformity*. It can scarcely be necessary to remind a geologist of extended practical experience, that in exploring the boundary lines of two conjoining formations of different æras, the two may be found both in conformable and in unconformable position with each other in different portions of their expanse; the extent of conformity or unconformity in any one part depending on the form and direction of their respective stratification in that part. But in determining the relative age of the two formations, I consider unconformity (on however small a scale developed in a given district) as the rule for our guidance, (especially when supported by corroborative considerations,) the conformity in such case arising merely from coincidence in the stratification. As an instance, I will refer to the position of the Old Red Sandstone of Gloucestershire in relation to the subjacent transition beds. In the environs of Tortworth, the strata of the two formations are in the eastern quarter of the field in conformable position, while in the western and north-western quarters they are for the greater part unconformably disposed*. Similar relations

* Geological Transactions, vol. i. second series. Geological observa-

appear also in the N.W. of Gloucestershire and adjacent parts of Herefordshire*. Now, no one contends that these formations are of the same epoch, but quite the contrary: the Old Red Sandstone here also, be it observed, containing no fossils of the transition æra, nor any other except some vestiges of vegetable remains. And if we refer to Ireland, similar observations may be made: thus, for example, the same body of Old Red Sandstone, which surrounds the Bilboa mountains, lies near Newport unconformably, and at the outlet of the Bilboa river conformably, on the subjacent clayslate; arising in fact from the variable position of the clayslate strata, while that of the Old Red Sandstone is more constant, being disposed in extended overlying sheets †. It appears needless to multiply instances. In all such cases unconformity must assuredly be taken as the governing principle in deciding relative age.

Now, to apply this to North Devon. While admitting the generally apparent *parallelism* (namely, conformity of strike, but not necessarily of dip) between the beds of the carboniferous series and the subjacent Trilobite slates, I have adduced two examples of what appeared to me non-conformity; the one in Runson Lane on Rosa Hill, South of Barnstaple, the other in the region of the Pill near Muddlebridge on the south side of the harbour. In Runson Lane the relations on which I rely may not be very obvious to a passing explorer, as they are not so directly apparent in the road as in the bank above, and even there the relations are partially concealed by soil and herbage, and require close inspection. Here the coarse shale, which abuts against the Trilobite slate (both dipping south), is inflected to the north, and forms an arched curve overlying the latter, and therefore unconformable. The space exhibited is certainly not considerable, yet seemed to me sufficient to determine the fact, when duly examined; while the total dissimilarity in the characters, both mineralogical and zoological, of the coarse shale and the Trilobite slate, is a corroborative. In this part of the section laid before the Geological Society, I did not attempt a finished drawing of the accompanying soil, herbage, &c., but simply expressed the fact of unconformity. In the

tions on parts of Gloucestershire and Somersetshire. § § 6, 10, 17, with the Map and Section, No. 2.

* Geol. Trans. § §. 34. 35. and Section No. 4. Both these examples of unconformity are admitted by Mr. Murchison; see pp. 444, 456, of the Silurian system; a work, in the execution of which a most important contribution has been made to geological science, affording ample evidence of the unwearied zeal and diligence of the author.

† Geol. Trans., vol. v. Memoir on the East of Ireland, §. 139.

other case, near Muddlebridge, I derived the unconformity partly from observation, partly from inference. There the Fremington limestone and shale form an arched curve, broken at the surface, supporting on each side the sandstone of the coal-measures, the latter being in mass on the southern side, but appearing only as a remnant on the northern side; in the former case dipping 70° to the S.W. and in the latter 50° to the N.E. The immediate contact of these carboniferous strata with the subjacent Trilobite slates is not visible, but in the nearest adjoining portion of the latter formation exposed to our view, namely, in the Bickington quarries on the east, we find the dip of the beds to be 60° to the southward. If then we suppose, as we are justified in doing, that these beds extend to the westward under the carboniferous strata near Muddlebridge, the unconformity of the two formations becomes manifest. The nearest Trilobite slates seen on the north show also a southerly dip. It was to illustrate these relations that I constructed the diagrams of a plan and section in the region of Muddlebridge, as laid before the Geological Society.

On the other hand, if we proceed to the southern side of the Devonshire carboniferous trough, it is admitted that in one case at least an unconformity is observable between the carboniferous series and the subjacent schistose rocks*, although a general coincidence or parallelism in the strike of their respective strata may be apparent adjacent to their boundary line; but the culmiferous series in a part of its extent is said to rest also upon granite. Mr. Austen, however, has repeatedly stated that the culmiferous measures in South Devon rest unconformably upon a series of deposits belonging to the transition system†; and some of the remarks of Mr. De la Beche in that quarter are much to the same effect‡.

That the carboniferous group of Devon should be directly deposited on rocks of the transition epoch, without the intervention of the Old Red Sandstone, should not surprise us, since analogous occurrences may be noticed in other parts of the island, e. g. in those portions of Pembroke, Westmoreland, and Cumberland where the Old Red Sandstone is wanting; so also in the South of Ireland. Nay, in some quarters of the world coal tracts are found reposing immediately on granite, e. g. in France and in Virginia, U.S. From the mere

* Proceedings of the Geological Society, May 1838, p. 681; and Lond. and Edin. Phil. Mag., vol. xiv. April 1839, p. 246, note.

† Proceedings of Geol. Society, Dec. 1837, pp. 586, 588; and Ibid. April 24th, 1839.

‡ Geol. Report on Cornwall, Devon, and W. Somerset, pp. 61, 107, 111.

juxta-position of formations, all will admit that we cannot draw a direct inference as to their relative age.

Thirdly, as to the *relative age* of the Older Stratified Rocks of North Devon, extending from the Trilobite slates near Barnstaple on the S. to the Foreland sandstone on the N. In now considering these deposits as the equivalents of the Old Red Sandstone, the authors appear to have been led partly by relying on an apparent conformity and gradation between the Trilobite slates and the superincumbent carboniferous rocks, and partly and chiefly from observing that in these Older Stratified Rocks, though distinguished by many transition fossils, are yet to be found certain Spiriferæ, Productæ, and Terebratulæ, with perhaps some other organic exuviæ, similar to such as occur in the carboniferous series; at the same time throwing overboard all regard for the mineralogical distinctions which are observable in these Older Stratified Rocks*. With respect to the two first points, the supposed conformity and gradation, I have already expressed the grounds of my dissent; and with regard to the third, the occurrence of certain Spiriferæ, Productæ, and Terebratulæ, it may be remarked, that so far back as the year 1824 † I intimated that some species of those genera are common both to the transition and carboniferous epochs; and in my observations on the South of Ireland between that year and 1830, and renewed in the year 1834, I was confirmed in that view, and which has been since set forth in my Memoir on that portion of the island (published in 1837), in which I have shown that in particular several species of those genera, besides others, occur in various countries both in the transition and carboniferous systems ‡. From analogous occurrences in the limestones of South Devon, at Plymouth, &c., Professor Phillips was induced in 1833 to express doubts as to whether those

* Governed by these views, the authors propose the term *Devonian System*, to comprise "all the great intermediate deposits between the Silurian and Carboniferous systems." (Lond. and Edin. Phil. Mag., vol. xiv. p. 259. April 1839.) But, it may be asked, what evidence have we of this *intermediate* position? Where do we find those deposits resting on the Silurian system, and passing upward into the old red sandstone?

† Geol. Trans., vol. i. second series. Observations in Gloucestershire and Somersetshire, §. 9. p. 329.

‡ Geol. Trans., vol. v. second series. Memoir on the South of Ireland. §§. 13, 33, 57.—N.B. In that memoir I did not employ Dalman's genus of *Atrypa*, as including certain Spiriferæ and Terebratulæ, Von Buch having shown that Dalman's distinction was founded in a mistake. (See *Ueber die Terebrateln von Leopold von Buch*, p. 23. Berlin, 1834.) Mr. J. de C. Sowerby, however, adopted that genus. (See Min. Conchol., Aug. 1835. See also Silurian System, pp. 643, 644.)

limestones really belonged to the transition system*. To these doubts I adverted in a note to the 33 §. of my Memoir on the South of Ireland. Analogous relations appear also to exist in the transition countries adjacent to the Rhine, and the works now in progress to illustrate the organic exuviæ found in those tracts may assist in throwing additional light on this subject†.

This case, however, is not a singular one in geology, it being well known that in the instance of any two systems following one the other in the geological progression, although distinguished respectively by peculiar organic remains, yet certain species occur common to both systems. The partial appearance therefore of certain species among the fossils of transition strata which are common in the carboniferous series also, does not appear a sufficient reason for including such transition strata in the formation of the old red sandstone ‡. Still less could any such inference be drawn from a consideration of the component rocks, for, taken as a whole, none can be more dissimilar than the Older Stratified Rocks of North

* *Encyclopædia Metropolitana*, art. Geology, pp. 577. 578.

For further details of the fossils found in the Older Stratified Rocks of North and South Devon and Cornwall, see Mr. De la Beche's Report on Cornwall, &c.; namely, pp. 50, 51, on North Devon; pp. 57, 60, 64, 75, 76, on South Devon; and pp. 79, 81, 82, 83, 86, 87, 89, 90, 91, on Cornwall; the authorities referred to being Professor Phillips and Mr. J. de C. Sowerby. And compare with these details those given by Prof. Sedgwick and Mr. Murchison in Lond. and Edin. Phil. Mag. for April 1839.

† See Dr. Beyrich's *Beiträge zur Kenntniss der Versteinerungen des Rheinischen Uebergansgebirges*; and also *De Goniatites in Montibus Rhenanis occurrentibus*; works published at Berlin, April 1837. [An English translation of this memoir appeared in the Annals of Natural History, No. XIV. for March 1839. Ed.]

‡ This doctrine appears to me to be supported by Mr. Murchison himself, when judiciously remarking:

" If, therefore, it should prove after all that a *few* species of *conchifers* continued in existence, from the formation of the Silurian Rocks to the accumulation of the carboniferous limestone, how can their presence break down the individuality and separation of systems, established upon such a vast preponderance of direct zoological evidence in the other natural classes? Even should a few other *mollusca* in the two systems be considered identical, there is no doubt, that by far the greater number of them which truly belong to rocks rising from beneath the Old Red Sandstone, are distinct from those which inhabit the strata above that system.

" Such evidences are therefore nothing more than additional supports of the important truth which geology has already established, that each great period of change, during which the surface of the planet was essentially modified, was also marked by the successive production and obliteration of certain races.

" Let it not, however, be imagined that I wish to inculcate the doctrine, of every ancient formation having been tenanted by creatures absolutely peculiar to it. The large natural groups of strata only, or, so to speak, systems, can be thus distinguished." (*Silurian System*, p. 582.)

In this general view I quite agree.

Devon, and those which have hitherto been met with and described as constituting the Old Red Sandstone formation. If in attempting to remove this difficulty, namely, as arising from the difference of mineralogical character, we have recourse to *metamorphism*, what is there gained by such an assumption? We can hardly suppose any very intense development of heat in the present instance. But supposing the highest state of excitement, that of *igneous action*, could we expect other than an intimate chemical combination of the constituents of the rocky masses, and a new arrangement of the molecules according to the laws of their polarities? What evidence have we of any such intensity of action in the present case? Not in the mineral characters certainly, nor are the organic remains which are distributed in the series obliterated; on the contrary, they are abundant, and in many cases well pronounced. And the rocks of which the group consists, and to which a general allusion has already been made, are analogous to such as are to be found dispersed in other transition tracts. Thus, if the Older Stratified Rocks of North Devon be compared with those occurring in certain portions of the South of Ireland, such analogies doubtless present themselves. There are these differences, however, between the two tracts; that a much greater diversity of rocks occurs in the broad expanse of the South of Ireland, than in the confined district of North Devon, including various conglomerates among the number (and in this respect corresponding likewise with the relations in the N.W. of Gloucestershire, and adjacent parts of Herefordshire*:) while, on the other hand, the organic remains distributed in the Old Stratified Rocks of North Devon appear to be much more abundant and in greater variety than I have noticed in any part of the South of Ireland. Now, admitting such analogies to exist, let us carry the comparison somewhat further, and see to what it leads. These rocks in the South of Ireland are overlaid in many instances by detached groups and mountain masses of the Old Red Sandstone formation, in decided unconformed position. The county of Waterford may be cited as affording numerous examples. And in none of these cases do we find anything like a passage from the transition series to the Old Red Sandstone formation above†.

* See note to §. 34. of my Memoir in the Geological Transactions, vol. i. second series.

† I have in numberless instances shown that in Ireland the Old Red Sandstone formation reposes on the Older Stratified Rocks, both of the transition and primary epochs, in discordant position. Nothing can be more abrupt and distinct than the unconformity of the Old Red Sandstone there displayed. And nowhere have I perceived any gradation between the Old Red Sandstone and the older rocks. (See Geol. Trans., vol. v. Memoir

If then the Older Stratified Rocks of North Devon be allowed to bear analogy to those in the South of Ireland, the relations now stated appear to be conclusive as to their relative

on the East of Ireland, §§. 134 and 135. §§. 137 to 139, and §§. 143 to 151: and Geol. Trans., vol. v. second series, Memoir on the South of Ireland, §§. 10, 13, 20. §§. 47 to 49, and §. 69.) That the same unconformed position prevails also in Scotland and in the North of England is admitted by Prof. Sedgwick. (Proceedings of Geol. Society, May 1838, pp. 676, 678.)

All these instances prove a positive break in the geological succession between the transition and the carboniferous epochs.

How is this fact, based upon a wide field of observation, to be reconciled with that solitary example of gradation which is described by Mr. Murchison as taking place from the Silurian system into the Old Red Sandstone formation, and his consequent division of the latter into three masses numbered in descending order, 1. conglomerate and sandstone, 2. cornstone and marl, 3. tilestone? Perhaps the following view may assist toward removing the seeming discrepancy. Let us consider the subject in reference to relative position, composition, and organic remains.

With respect to position, if we look at the map we perceive at once that this great deposit is flanked on both sides, on the E. and W., by Silurian rocks, while protruding masses of the latter are contained within its area. It is therefore clearly spread in one broad sheet (with a few interruptions) over the Silurian rocks as a base. But is this upper deposit strictly conformable throughout to the lower? From Mr. Murchison's descriptions it evidently is so on the Western flank, and in many instances in the interior also, being in the latter case disposed in conformity to the undulated structure of the subjacent Silurian strata. Yet in those parts where the stratification of the Old Red Sandstone is uninterrupted, it appears arranged upon one inclined plane, unto its junction with the superincumbent carboniferous limestone, dipping throughout toward the East, as represented in pl. xxxi. fig. 1. On the Eastern flank of the field, however, where the Old Red Sandstone comes again in contact with the Silurian rocks, there are clear instances of unconformity between the two. The disposition of the Old Red Sandstone, therefore, though apparently mainly governed by the form presented to it in the surface of the Silurian rocks, has not in all cases been regulated by it; and this circumstance, combined with its widely overspreading the Silurian rocks, may be considered a valid proof of the difference of age, agreeing in that respect with observation and experience in other tracts.

This inference refers more particularly to the upper divisions, the cornstone and marl, and the conglomerate and sandstone; for it does not appear strictly to apply to the lowest division, to the tilestone; and for the following reasons. The two upper divisions find their analogies in other parts of the world, the uppermost as being non-fossiliferous (with the exception of a few vegetable remains sparingly scattered), the lower as containing the remains of fishes; but the lowest division, the tilestone, has not met with its parallel elsewhere, being distinguished by its fossil shells. It thus appears as an anomaly. It is of much smaller dimensions than the overlying formations, and reposes directly on the Ludlow rocks, and, while containing some peculiar fossils, exhibits also others similar to those of the Ludlow formation. Its affinity to the latter is thus fully shown, while in the cornstone not one of these shells is found. Again, the fossiliferous

age; as to their being in fact both referable to an epoch antecedent to that of the Old Red Sandstone formation, and in nowise connected with the latter as terms of the same series.

tilestone appears to be confined to the Western flank of the Old Red Sandstone, serving as a base to the cornstone division: for on the Eastern side, in the instances cited of flagstones between Mathon and Ledbury, and at the N.E. suburb of Ledbury, it is not mentioned that these flagstones contain there any fossils; and hence they may prove to be mere modifications of Old Red Sandstone in that quarter. The continuity of the fossiliferous tilestone through the field, therefore, does not appear to be proved. The observations made in the Clytha hills N. of Usk, (pp. 438, 439,) seem to me to relate merely to modifications of the upper Ludlow beds; a simple change of colour from gray to red being quite unimportant.

Analogy thus pointing to the Silurian system rather than to the Old Red sandstone, is it venturing too far to suggest that the fossiliferous tilestone might have been more appropriately included in the Silurian system? For, entering more into particulars, let it be considered that of the twenty-seven species of shells enumerated as being found in the fossiliferous tilestone, seven species are marked as occurring in the upper Ludlow rock also (figured in Pl. 3, 5, and 19, and described in pp. 602—604. See also p. 183.) And to this statement I may add, (although I have not my Tortworth fossils lying before me, so as to speak with precision in all the cases) I recognise several that are common in that Silurian district, especially at Long's Quarry in Charfield Green, which quarter is designated by Mr. Murchison as Caradoc sandstone. And with respect to the remains of fish, which occur more particularly in the cornstone division, and to a less extent in the tilestone, it may be observed that, while some species are peculiar to each of those divisions, " the Ichthyodorulites of the Old Red Sandstone belong to distinct species of the genera *Onchus* and *Ctenacanthus*; but some species of these genera exist in your Carboniferous, Old Red, and Silurian systems," (p. 596. Observation of Mr. Agassiz to Mr. Murchison). The most marked distinction therefore consists in the shells found in the tilestone division.

From the preceding considerations the question arises, whether it would not be a more natural view to consider the Old Red Sandstone formation, properly so called, as restricted to that great mass which is composed of Mr. Murchison's upper and central divisions, preserving thus the analogy with other countries; the upper division, the conglomerate and sandstone, being wholly non-fossiliferous (with the exception, as before stated, of a few vegetable remains), and the under division, the cornstone and marl, distinguished by the presence of the remains of fishes and the absence of shells. With respect to the fish remains in the cornstone, the analogy also holding good in Scotland and the Orkney Isles, it may become an interesting object of research to ascertain, whether they occur also in the marly clays of the Old Red Sandstone formation in Ireland.

Considered in this point of view, and thus restricted, the Old Red Sandstone formation may be considered, as it has hitherto been, as the first member of the carboniferous series, which, being most commonly unconformable, and forming a break with the Older Stratified Rocks, marks the commencement of a new epoch; and serving as a base to the succeeding deposits of the carboniferous limestone and coal-measures, these three are often interstratified one with the other in the order of succession. They thus constitute together a natural group, which, in conformity with the view hitherto entertained, has been very appropriately designated the

And if such be the case, the same may be predicated of the Older Stratified Rocks of South Devon and Cornwall also, since similar relations are stated to exist in those districts as in North Devon. And this conclusion will bring us back to the position formerly entertained, and I believe correctly; namely, that a large portion of the Older Stratified Rocks of South Devon and Cornwall belong to the transition system *. But to what portion of that system? it may be asked. The question implies the impression of a determinate order of succession in the transition series. Does such a sequence strictly prevail in nature, so as to admit of general application? I do not conceive that an affirmative reply would be correct, if we may judge by what we have hitherto learnt from the description of our own and other countries. And in avowing this persuasion, far be it from me to disparage in the slightest degree the assiduous and invaluable labours of Prof. Sedgwick and Mr. Murchison. The Silurian system of the latter cannot be too highly appreciated, as no doubt will also be the case with the Cambrian system of the former, whenever the public shall be gratified by the appearance of that work in all its details. As comprising an able development of the structure and composition of the districts described, together with the fossils by which they are characterized, as well as of the disturbing forces by which they have been affected, the Silurian system will always maintain a high rank in geology. Yet, if we duly consider the tumultuary throes under which the arrangements of the transition epoch appear in general to have taken place, and the consequent uncertain and fluctuating disposition of the mineral masses composing the system, with a distribution of organic remains much depending on the me-

carboniferous series. But Mr. Murchison would now restrict the *carboniferous system* to the carboniferous limestone and coal-measures merely. In so doing, I think, he departs from nature. Time will prove which of the preceding views may be the more correct one.

* To designate these rocks as the representative or equivalent of the Old Red Sandstone formation strikes me as singularly inappropriate; that is, if we continue to retain any respect for mineralogical distinctions, and are not carried away by hypothetical views. A practical man does not so readily conceive that a glossy clayslate, for example, in association with a variety of other rocks, and such series containing numerous fossils, can be the equivalent of the Old Red Sandstone formation, properly so called, wholly destitute of these fossils.

I mean to say that mineral characters are always entitled to a fair share of our consideration, in combination with organic remains and relative position, when judging of the relative age of rocks; and that to exclude from our view either one or the other of these marks of distinction, is to run the risk of falling into error, and confounding subjects really distinct.

dium in which they are enveloped, as being of an argillaceous, calcareous, or siliceous nature, we can hardly expect to find that the local arrangements of one country should strictly correspond with those of another*. I am persuaded that many general resemblances exist, but that a diversity prevails in the details of different tracts, both with respect to the composition and disposition of the mineral masses and the distribution of the remains of organized bodies. In this view we appear to be borne out by all that is hitherto known of the transition relations of Ireland, England, Scotland, Scandinavia, Russia, Poland, Germany, Belgium, France, or North America. However, let future and more extended inquiries decide this question. In the mean time it may be remarked that, while in these different countries many of the organic remains correspond as to species, yet peculiar species also occur in some of those tracts which have not yet been found in others. The comprehensive general views expressed under this head by Professor Sedgwick appear to me so truly just and appropriate, that the following extracts from his concluding words may be very appositely introduced in this place.

"Some of the generalizations are, however, founded on imperfect evidence, and to render them more complete, it is now necessary to appeal to the organic remains in the several groups. In this department little has been yet effected, excepting in the higher part of the Silurian system, where the upper divisions (at least in one part of the island) assume definite mineralogical and zoological types. Whether definite geological groups can be made out in any lower system still remains to be seen. The difficulty of classification by organic remains increases as we descend, and is at length insurmountable; for in the lowest stratified groups, independently of metamorphic structure, all traces of fossils gradually vanish; and the great range of certain species through numerous successive groups, and the very irregular distribution of fossils even in some of the more fossiliferous divisions, add greatly to the difficulties of establishing true definite groups even within the limits of our own island. The difficulties are indefinitely increased in comparing the formations of remote continents. But these circumstances are compensated by the magnificent scale of development of the successive groups, and their wide geographical distribution. Taken together

* I perceive that on this subject there is nearly a coincidence between Mr. De la Beche's views and my own, formed independently of each other. See Report on Cornwall, &c. p. 39.

they have a great unity of character *. To these views, so clearly expressed, I fully subscribe.

Upon the whole, there appears to me no adequate reason for considering the Older Stratified Rocks of Devon otherwise than as a portion of the transition system; for the occurrence of certain fossil plants in the Wollacomb and Marwood bed of sandstone (subjacent to the Trilobite clayslate), even should they really be found to bear analogy to some that are met with in the coal-measures, does not seem sufficient to invalidate this conclusion. It is much to be desired that the fossil plants derived from the Marwood sandstone should undergo a rigorous examination as to the different genera and species of which they may consist, especially as the evidence hitherto given in respect of them is in some degree conflictive †. It may be remarked, however, of these sandstones, that among them none of the fern tribe have been met with, whilst these are of frequent occurrence among the Devon coal-measures. That fossil plants occur also in the South of Ireland, among the transition strata at Dunmore, in the county of Kerry, as noticed by the late Mr. Alexander Nimmo, I have already placed on record ‡; and I have to express my regret that I received that information subsequent to my visits to that part of the island. It is very desirable that some competent geo-

* Proceedings of the Geol. Society, vol. ii. No. 58, pp. 683, 684. May 1838.

† Professor Lindley is stated to be of opinion that none of these plants derived from the Marwood and Wollacomb sandstones are similar to those which are common to the Devon culm-measures: some resemble decorticated *Lepidodendra* and others *Sternbergia*? One specimen resembles *Calamites Voltzii* of the terrain d'anthracite inférieur (Voltz). (Proceedings of Geol. Society. Prof. Sedgwick and Mr. Murchison, in vol. ii. No. 51. p. 559.) But such of the plants occurring in the Devon culm-measures near Bideford, &c. as were determinable, had been identified by Professor Lindley with species characteristic of the true coal-measures. (*Ibid.* the President's addresses in No. 39, p. 163, and No. 49, p. 491. And Prof. Sedgwick and Mr. Murchison in No. 51, pp. 561, 562.)

It has, however, been also stated that some of the plants of the Wollacomb sandstones are supposed to be true carboniferous plants, while all the plants in the culm-measures are described as identical in species with plants of the carboniferous series. (*Ibid.* Prof. Sedgwick in No. 58. p. 681. May 1838.)

And Prof. Sedgwick and Mr. Murchison repeat that some of the plants from the Marwood, &c. sandstones are considered by the Rev. David Williams and Mr. De la Beche, on the authority of Dr. Lindley, as undistinguishable from plants of the carboniferous system. (Lon. and Edin. Phil. Mag., vol. xiv. p. 243. April 1839.)

See also Mr. De la Beche's own statement, in reference to both these subjects, in the Report on Cornwall, &c., pp. 50 and 126; the authority of Prof. Lindley being in both cases referred to.

‡ Geol. Trans., vol. v. second series. Memoir on the S. of Ireland, §. 13.

logist should strictly examine that quarter, and give us the result of his researches. But in such an occurrence there appears to be no real novelty, as analogous relations have been stated to exist in some parts of the continent *.

Should Professor Sedgwick and Mr. Murchison, or Professor Phillips, as proposed, execute the plan of a full description and figuring of all the undescribed fossils met with in the Older Stratified Rocks of Devon and Cornwall, they will add greatly to the obligations already conferred on geology by their valuable labours. And with respect to North Devon, it may not be immaterial to bear in mind the observation of the former gentlemen, that some of the organic exuviæ met with in the calcareous slates at Linton, which lie low down in the series, are specifically similar to others appearing in the uppermost part of the series, namely, in the Trilobite slates at Barnstaple †. This task, when accomplished, will put to the proof to what extent these tracts agree or disagree with other transition tracts hitherto described.

In now turning again to Ireland, I do not advert particularly to the concluding paragraph of Professor Sedgwick and Mr. Murchison ‡ (in which they refer to certain observations made in Ireland by Mr. Charles W. Hamilton), for this simple reason, that I do not clearly comprehend their application; and Mr. Hamilton's paper I have not seen. There appears to me some confusion of terms in the case.

But Mr. Murchison having in his Silurian system, (pp. 580, 581,) indulged in some strictures in disparagement of the conclusions to which I had been led in the South of Ireland, I feel called upon to offer the following in reply; first giving the statement of the author.

The remarks of Mr. Murchison refer more particularly to the great band of limestone that stretches past Cork, which is intercalated among the transition rocks of that quarter; on which he observes, "Mr. Weaver arrives at the conclusion that a large number (between 60 and 70 species) of the fossils of the transition and carboniferous systems are *identical.* My zoological data and inferences are completely at variance with those of Mr. Weaver." That is, of course, taking the Silurian system as a standard measure of all other transition tracts. But it has already been seen that, however highly pleased with the Silurian system itself, I am not content to

* For some instructive details on this subject, the reader may consult Mr. De la Beche's Report on Cornwall, &c. pp. 132—136.

† Lond. and Edin. Phil. Mag., vol. xiv. p. 244. April 1839.

‡ Ibid. p. 260.

receive that system as an infallible guide when we step out of the Silurian region, and pass into other tracts of transition or protozoic origin.

Again, "Mr. J. de C. Sowerby is of opinion that all the fossils therein enumerated (p. 21.)* as belonging to the *transition* limestone of Cork are, with *one* exception, characteristic fossils of the carboniferous limestone of England and Ireland, and *therefore* that they are of the *same* geological age as those which in another part of the memoir are described as *exclusively* belonging to the carboniferous strata." I must here remark that, *not exclusively* would have been a more correct expression, several of the species being there marked as common to transition countries also.

"Judging from the printed lists, Prof. Phillips also thinks that the Cork limestone fossils are carboniferous." And "Mr. Sowerby agrees with me in believing that the only fossils alluded to by Mr. Weaver, which really belong to the more ancient rocks (Silurian, &c.), are those enumerated (pp. 10. 15. et seq.)."

Again, "From an inspection of Mr. Weaver's map alone, I cannot avoid surmising that the localities where true Silurian fossils might occur, are those alone where such *have really been* detected, as the strata in those situations are separated from the carboniferous limestone by large masses of Old Red Sandstone. (See Smerwick Harbour, &c., on the coast of Kerry, and the coast of Bonmahon River, Waterford.) The Old Red Sandstone, that important feature of separation, being wanting (on the south) in all the remainder of the country described, is it hazarding too much to suggest, that some of the limestones which there occur, and which are loaded with carboniferous fossils, may be outliers and remnants of the base of that system, which we know to be so vastly expanded and widely diffused throughout other parts of Ireland?"

The reader being thus put in possession of Mr. Murchison's view of the case, I must now entreat his attention to my own.

I have to observe in the *first* place that the number of fossil species which I have enumerated as occurring in the Cork great band of limestone, is *forty-eight*; of which *twenty-five* species are referable to the genera Spirifera, Terebratula, and Producta.

2ndly, Of the other fossils there are *seven* species which have been found in the Cork limestone alone; namely, *Nautilus funatus*, *N. compressus*; *Euomphalus triangularis*, *E. ovalis*; *Ampullaria ovalis*; a species of *Solarium*; *Isocardia oblonga*.

* Memoir on the South of Ireland in Geol. Trans., vol. v. Second Series.

3rdly, But there are *twenty-four* species which occur both in the Cork band of limestone and other transition countries; of these, *three* species are included in Mr. Murchison's List of Silurian Fossils, derived from the Wenlock limestone, viz. at Ledbury and Dudley; and two of these occur also at Plymouth, and one in the Eifel and Gothland: they are, *Nerita spirata*, *Spirifera radiata*, and *Spirifera octoplicata*; and *three* others, also derived from the Wenlock limestone, viz. at Dudley, but not included in Mr. Murchison's list; and one of these is found at Blankenheim: viz. *Spirifera striata*, *Euomphalus pentangularis*, and *E. catillus*. *Six* other species in the Cork limestone are met with also in home transition localities; namely, in the Isle of Man, at Ludlow, Plymouth, and Newton Bushel; and three of them occur also in foreign transition countries, viz. in the Eifel, at Coblentz, and Blankenheim: they are, *Pileopsis vetusta*; *Producta Scotica*; *Spirifera distans*, *S. cuspidata*, *S. rotundata*; *Terebratula pugnus*: *twelve* other species occur in foreign transition countries, viz. *Orthoceras striatum*; *Nautilus globatus*, *N. ovatus*; *Spirifera oblata*, *S. pinguis*; *Terebratula crumena*, *T. sacculus*, *T. lateralis*, *T. lævigata* (Schlotheim), *T. acuminata*; *Amplexus coralloides*; *Actinocrinites* 30-*dactylus*: these foreign localities being Norway and Sweden, Christiania, Malmoe, Gothland, Hof, Schleitz, Blankenheim, Eifel, Coblentz, Bensberg, Ems, Mont Chatou near Coutances, and the Catskill Mountains in the state of New York; countries admitted to be of protozoic origin, and to which Mr. Murchison himself in a great measure refers, when giving an account of his Silurian corals, viz. Sweden, Gothland, Bensberg, Eifel, and the transition regions adjacent to the Rhine, both in Germany and Belgium; as also to Lake Erie and Drummond Island in North America.

4thly, But there are *seventeen* species in the Cork band of limestone, which have hitherto been noticed elsewhere only in the carboniferous limestone; viz. *Asaphus gemmuliferus*; *Orthoceras Breynii*, *O. fusiforme*; *Nautilus multicarinatus*, *N. cariniferus*, *N. sulcatus*; *Ampullaria helicoides*; *Producta Martini*, *P. concinna*, *P. lobata*, *P. punctata*, *P. antiquata*, *P. plicatilis*; *Spirifera trigonalis*; *Terebratula resupinata*, *T. cordiformis*; *Pleurorhynchus hibernicus*.

In further illustration of this subject, I also showed, in the notes to §. 33. of my Memoir on the South of Ireland, that of the genera Producta, Spirifera, and Terebratula, no less than *eighteen* other species had been observed, partly in other portions of the British Isles and partly in foreign countries, as common both to the transition and carboniferous systems;

namely, *Producta depressa*, *P. hæmispherica*, *P. longispina*, *P. sulcata*, *P. anomala*, *P. sarcinulata*, *P. scabricula*; *Spirifera glabra* and *obtusa*, *S. attenuata*, *S. ambigua*, *S. reticulata*, *S. decurrens*, *S. striatula*; *Terebratula Mantiæ*, *T. platyloba*, *T. plicatella*, *T. diodonta*, *T. affinis*; and the transition tracts to which these are referred are, at home, Dudley, Newton Bushel, Plymouth, and South of Ireland; and abroad, Christiania, Gothland, Eifel, Coblentz, Bensberg, Blankenheim, Ems; and Catskill Mountains, Albany, and Trenton Falls, in the State of New York.

There is no gainsaying these facts, the lists having been prepared with great care; and I consider Mr. Murchison not at all justified when he applies to this part of my memoir the following remark: "Still less can we admit the validity of arguments founded, either upon mere lists of fossils which may have often been erroneously identified, or simply upon the names attached to formations by geologists who have not studied the whole sequence of the deposits in question." (p. 580.)

With respect to the first part of this remark, bearing on the identification of species, foreign naturalists may not receive it as any great compliment; but I confess I am not so sceptical myself as to undervalue their discriminating powers; at home, luckily, the identification will not be disputed, the principal authority referred to being Mr. J. de C. Sowerby. And with regard to the second part, relating to the formations in which these fossils have been found, the best answer that can be given is to point to the native and foreign localities which I have already enumerated.

I must also remark that Mr. Murchison does not correctly state the case when he represents that some of the limestones in the south of Ireland are loaded with carboniferous fossils. The list given by me in §. 33. relates to the Cork great band of limestone described in §. 29. Of all the other bands, those described in §§. 22. to 28. inclusive, and in §§. 31. and 32. exhibit organic exuviæ very sparingly; the principal being Crinoidal remains, a few Spiriferæ and Productæ (and among these the *Producta depressa*), an Avicula, a Goniatite, and very few Polyparia. But in the two bands of limestone, described in §. 30., which are interstratified with and flank the interposed mass of clayslate of which Rinniskiddy Hill mainly consists, situated on the western side of the lower part of Cork harbour, fossils appear to be numerous; but no one, I believe, has hitherto given them that attention which they appear well to deserve. I trust some competent geologist will take the subject in hand.

I must further observe, in reference to the last quoted re-

mark of Mr. Murchison, that my conclusions were not founded upon mere lists of fossils, but upon their matrices forming regular intercalated beds in a decided transition country in the south of Ireland; relative position having, in all cases, been the primary consideration, both there and in the other native and foreign localities referred to.

No doubt all this does not exactly correspond with the relations so ably developed in the Silurian System; but it tends to bear me out in my argument that, in widely extended, or distant separated, lands of protozoic origin, the relations, though akin, may not in all cases be precisely alike. If further proof were wanting, we have only to step across the Irish Channel, and enter Devon and Cornwall, where the relations, be it observed, appear to me to be much more nearly allied to the South of Ireland than with the districts described in the Silurian region. But the affinities are, perhaps, still stronger between the South of Ireland and the transition tracts bordering upon the Rhine, than with any of the English districts mentioned, if we may judge by what is already known of those tracts.

With one observation of Mr. Murchison I fully concur. "The only effective remedy," he remarks, "for the scepticism engendered by loose comparisons is to publish monographs, with figures of all the remains found in each group of deposits, the stratigraphical limits of which have been precisely defined by competent observers," p. 580.

If Mr. Murchison and Prof. Sedgwick will do this, both with respect to the Devonian and Cornubian, and Cambrian protozoic departments, geology cannot fail to derive great advantage from their united scientific labours; and if some other zealous, active, and competent geologists will undertake and execute the same task in those quarters of Ireland not yet described, and complete the views in those that have not been minutely explored, we may hope eventually to see a constellation of Cambrian, Silurian, Devonian, and Hibernian Stars, shedding, so far at least, a clear and steady light upon the protozoic regions of our geological world.

The subject of *Metamorphism* having been touched upon in a preceding part of this paper, I am led, in conclusion, to advert to the ingenious speculations of Professor Babbage* and Sir John F. W. Herschel†, concerning the subterraneous

* Proceedings of Geol. Soc., vol. ii. pp. 72—76., March, 1834; and the Ninth Bridgewater Treatise, by Professor Babbage.

† Proceedings of Geol. Soc., vol. ii. pp. 548—551., May, 1837. Ibid. pp. 596—598., January, 1838. Ibid. pp. 645—648., February, 1838.

oscillations of the isothermal surfaces of great temperature, founded on the assumption of an incandescent mass existing deep below the Surface of the Earth. If we adopt this theory, which appears to afford a fair starting-point, it may not be very difficult to conceive developments of heat in the bowels of the earth proportionate to the depths of the mineral deposits and the pressures to which they may have been subjected, during the progressive construction of the Shell of the Globe to its present form and state; but which differed in the degrees according to the conditions that prevailed at different epochs. That a portion of this pressure was due to an incumbent ocean cannot admit of doubt, since all mineral formations, from the fundamental granite upward to the most recent of the tertiary class, exhibit abundant phænomena attesting this immergence.

In all successive epochs, therefore, pending the building up of the Crust of the Earth to its existing form and condition, it may be said that the combined or reacting agencies of heat and water have never been absent; though modified and varied in intensity and force according to the circumstances which interfered at given periods.

Hence it appears that we are not strictly justified in pronouncing rock formations, of whatever æra, as of purely igneous or of purely aqueous origin; for as in all cases heat and water have been conjointly or conflictively set in motion, it becomes, in any case, simply a question of the relatively greater or less intensity and energy with which their respective forces have been combined or have reacted on each other; and as all formations of a date antecedent to the post-diluvial were (if we except the lacustrine) formed adjacent to, in, or under the Ocean, their various conditions of position and structure, both general and subordinate, appear referable primarily to the laws of chemical and molecular attractions, modified by greater or less developments of heat and mechanical agency, under the influence of hydrostatic pressure.

During the progress of the formation of strata in the ocean, and the incidental raisings and subsidences to which they may have been subjected by elemental combinations or conflicts, fractures, disruptions, and dislocations would follow as a consequence; and such movements may, in some of their results, have given rise to the origin of many veins. Hence, a distinction between primary disposition and structure, and successive displacements, appears legitimate; comprehending under the term structure, all crystalline, stratified, and concretionary arrangements of whatever description; whether slaty, tabular, prismatic, globular, columnar, or otherwise.

Drawing then a distinction between original formation and contingent derangements, between primary construction and subsequent upraisings and depressions, if we look to the terms of the series exhibited to us in the existing relations of the constituent masses of our Continents and Islands, the vast body of cumulative evidence supplied by the extended researches of geological observers in general, among whom the name of Lyell stands conspicuous, seem to leave little doubt that the solid materials of the Shell of our Globe have undergone oscillatory movements in elevation and depression in reference to the level of the sea. In some cases, these movements appear to have been gently and slowly, in others violently and rapidly progressive, while subject to alternating fits of comparative repose and energy, differing greatly in degree and extent; thus corresponding with the successive differences which are observable also in the position, composition, and structure of rocks, and in the remains of the organized beings by which they are respectively characterized*.

But, in truth, let it be confessed, that, through whatever means or secondary causes, acting during the course of a vast Cycle†, the Great Architect of the Universe moulded the Surface of the Earth by successive stages into its existing form and condition, adapted to the habitation and purposes of man, a scene fitted for the exercise and cultivation of his moral and intellectual faculties: the full scope and application of those

* I had at an early period (in the year 1818) conceived that the relative position of rocky formations, their general and subordinate structure, their inflections, interstratification, and inclusion in each other, and their frequent mutual connexion with contemporaneous and true veins, were simply referable to the opposite powers of chemical affinity and mechanical action, under the influence of hydrostatic pressure; and not to an expansive power from beneath[1]. Yet maturer reflection requires the admission, that chemical action and an expansive power are, to a certain degree, correlative, and not always in strictness to be disjoined; in other words, that an expansive power, though not the cause of original formation, may have been a concomitant, or one of the results.

† This view, as to *one vast Cycle*, would seem to accord with the suggestion of the Rev. W. Whewell, late President of the Geological Society, expressed in his very able and profound work, the History of the Inductive Sciences. See vol. iii. on the two antagonist Doctrines of Geology, p. 617 et seq.

The same opinion, though perhaps not specifically expressed, would appear to breathe through the admirable address delivered by the Rev. Adam Sedgwick, as President of the Geological Society, on the 18th February, 1831. And other names, of the highest distinction among our British geologists, might, I believe, be also quoted, as impressed with the same sentiment.

[1] Geol. Trans., vol. v. Memoir on the East of Ireland, §. 202. note.

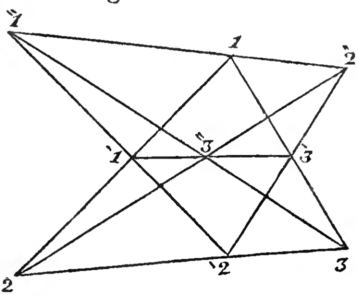
means, may in his present state of being always remain beyond the compass of his knowledge and understanding; although, seeing through a glass darkly, he be allowed to obtain partial glimpses of the truth, while engaged in the delightful task of endeavouring to trace the footsteps, and penetrate the benevolent designs, of the Creator, as manifested in His wonderful and infinite works.

XXIV. *On the Functional Symmetry exhibited in the Notation of certain Geometrical Porisms, when they are stated merely with reference to the Arrangement of Points.* By John T. Graves, *Esq., M.A., F.R.S., of the Inner Temple.**

Theorem I. Let $1, 2, 3, 1', 2', 3', 1'', 2'', 3''$ denote 9 points. In the following scheme, let a set of 3 points $1, 2, 1'$ lying *in directo* be denoted thus, $1\ 2\ 1'$. Then, if *any* 8 of the following sets be given, the remaining set will porismatically follow. (See fig. 1.)

$1\ 2\ 1'$	$1'\ 2'\ 1''$	$1''\ 2''\ 1$
$2\ 3\ 2'$	$2'\ 3'\ 2''$	$2''\ 3''\ 2$
$3\ 1\ 3'$	$3'\ 1'\ 3''$	$3''\ 1''\ 3$

Fig. 1.



The preceding theorem in all its generality may be derived as a *functionally formal* consequence, if we assume the two following propositions: 1. The order of succession of the given sets is immaterial. 2. There is *a certain* system of 8 sets out of the preceding 9, from which the remaining set will porismatically follow. The former proposition may be taken as a postulate from the nature of the subject which the symbols represent. In fact, the order of succession of the symbols in any single set is also immaterial. The second proposition requires geometrical proof. Assuming the former, it is easily seen that any one system of 8 sets may be put into the same form, with respect to its constituent symbols, as any other system of 8 sets, and any remaining set is formally derived from any one such system in the same manner as any other

* Communicated by the Author.

remaining set from any other such system. For example, instead of putting the set $3''\,1''\,3$ last, as was done in the preceding scheme, we may put $2'\,3'\,2''$ last, and we can get an arrangement which is a function of its constituent symbols similar to the preceding, thus:

$$\begin{array}{ccc} 3''\,1''\,3 & 3\,1\,3' & 3'\,1'\,3'' \\ 1''\,2''\,1 & 1\,2\,1' & 1'\,2'\,1'' \\ 2''\,3''\,2 & 2\,3\,2' & 2'\,3'\,2'' \end{array}$$

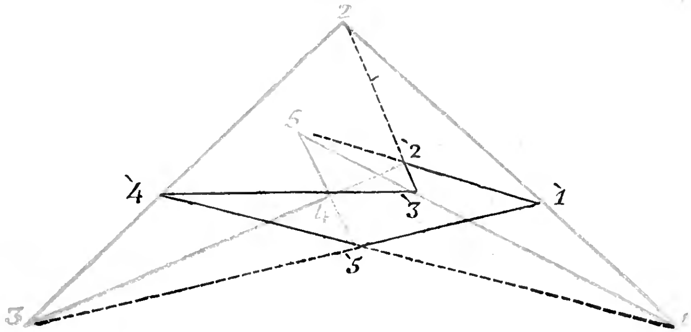
Theorem I. may be stated in various other ways, and fig. 1 will assume various appearances according to the position of the arbitrarily assumed points. It is interesting to examine the limits and relations of the arbitrary and the necessary according to the scheme. For example, take 6 points, arbitrary except in as much as they form 2 sets, and the remaining 3 points will be necessary, according to the scheme. Again, take 5 points, no 3 of which form a set, and the remaining 4 will be determined. Theorem I. is a particular case of a theorem due, I believe, to Pascal, viz. the intersections of the opposite sides of a hexagon inscribed in a conic section lie *in directo*. I may add, however, that, before the more limited proposition occurred to me, it had not, so far as I am aware, been treated as a particular case, or otherwise noticed. In fig. 1, the conic section always appears in the form of two straight lines. For example, the angles of the hexagon $1, 2, 2'', 2', 1'', 3$ lie alternately on the lines $1''\,2''\,1$ and $2\,3\,2'$, and the line $3'\,1'\,3''$ connects the intersections of its 1st and 4th, 2nd and 5th, and 3rd and 6th sides. I abstain from proving that those intersections, as a necessary consequence, lie *in directo*, because the proof would carry me away from the principal purpose of this paper, and proofs of Pascal's theorem are well known. The proposition relating to a hexagon inscribed in a conic section is a great acquisition to the geometry of the rule, as distinguished from the geometry of the compass treated by Mascheroni. Its converse enables us, being given 5 points, to trace out all the other points of a conic section by the mere intersections of straight lines drawn to and from given and arbitrarily assumed points without measurement of angles or distances.

Definition.—A polygon is said to *circumscribe* another when the angles of the latter lie upon the sides of the former (whether produced or not).

In fig. 1. the triangle 1, 2, 3 circumscribes $1', 2', 3'$, which circumscribes $1'', 2'', 3''$, *which circumscribes* 1, 2, 3 ! We have thus a ternary circulating series of mutual and simultaneous circumscription and inscription !

Theorem II. Let 1, 2, 3, 4, 5, 1`, 2`, 3`, 4`, 5` denote 10 points; then, if any 9 of the 10 following sets be given, the remaining set will porismatically follow. (See fig. 2.).

Fig. 2.



1 2 1`	1` 2` 5
3 4 2`	3` 4` 4
5 1 3`	5` 1` 3
2 3 4`	2` 3` 2
4 5 5`	4` 5` 1

Any one system of 9 sets out of the preceding 10 sets may be so arranged as to present the same functional form with reference to its constituent symbols, and to the derivation of the remaining set from it as any other system of 9 sets, e. g. if, instead of placing 4` 5` 1 last, we place 2 3 4` last, we may form out of the same sets the following scheme, which will be functionally similar to the preceding one:

4` 3` 4	4 5 5`
2` 1` 5	1 2 1`
5` 4` 1	3 4 2`
3` 2` 2	5 1 3`
1` 5` 3	2 3 4`

It may be remarked here, that if the first 5 sets in either of the two last preceding schemes be called x, and the second 5 sets be called y, and if y be considered as a function fx of x, we shall have $f^4 x = x$. What is the geometrical interpretation of this? Observe that the functional symbol f is here employed to denote similar internal changes in similarly constituted wholes, not operations in which the functional subjects are themselves similar constituent parts of similar formulæ.

Theorem II. may be stated in various ways. I believe it is best known in the following statement (see fig. 2). If a variable triangle (e. g. 2, 3\`, 4\`) whose sides always pass respectively through three given points (2\` 4 3) lying *in directo*, have always two of its angles (2 and 4\`) on two given straight lines (1 2 1\` and 4\` 5\` 1) its third angle will always lie on a third straight line (5 1 3\`). This proposition, which is not new except in form, admits of the following converse and extension. If a m-given lateral figure be inscribed in m-lines diverging from a point, while $m-1$ of its sides pass respectively through given points lying *in directo*, the remaining side will always pass through a fixed point.

In fig. 2. 1, 2, 3, 4, 5 and 1\`, 2\`, 3\`, 4\`, 5\` are pentagons *which circumscribe* each other! Fig. 2 is dotted and coloured for the sake of more clearly exhibiting this to the eye. If one pentagon and a point in one of its sides be assumed, the remaining points of the other pentagon will be necessarily determined by the scheme. It was by examining the scheme, not by inspecting the geometrical diagram, that I discovered the property of mutual circumscription. No triangle can be picked out of fig. 2, having 3 of the 10 points for its vertices, and having its 3 sides of the same colour.

I have stated the preceding theorems in a new form, for the purpose of directing attention to the symmetry and functional properties of the combinations of the symbols denoting points. The 9 sets in theorem I. and the 10 sets in theorem II. form *completed circuits of arrangement*, such that if in either case every set but one were given, the mind would by a natural induction from a perceived law of combination be led to supply the omitted set. This encourages the belief that, in cases where position without regard to magnitude is considered, recourse should be had to functional rather than algebraic equations, and that a systematic and manageable notation applicable to geometrical points might be devised which should not only express but *suggest* geometrical theorems.

Pondering on the nature of straightness, I have arrived at the following definition of a straight line. "Let a line be called straight, if it be ideally possible to divide it into two parts, each of which shall be of the same form as the whole." It is much more than enough to say, every part of a straight line is an exact model of the whole to which it belongs, and of every other straight line. The simple property stated in the definition is not possessed by any other line. Even circular and helical lines possess it not, although in the same circle or helix the curvature is the same at every point, and

the same lengths have the same form. For instance, the circular arc $\frac{2\pi}{3}$ is not a model of the arc $\frac{\pi}{3}$, nor of any other arc belonging to the same circle, and shorter or longer than itself. No mere magnifying nor diminishing will make any such shorter or longer arc agree in shape as well as length with $\frac{2\pi}{3}$. That the condition of the preceding definition contains in itself no contradiction, or, in other words, that straightness, so defined, is possible; that there is but one form of line which admits so much as *one* division possessing the required characteristic property; and that *every* division of the unique form equally possesses that property,—these are three true propositions, which seem to me to admit of proof, if we assume it as fact, or can prove independently, that there is a unique shortest distance between two given points.

Being once asked to arrange 19 trees in 9 rows, having 5 trees in each row, I found, upon trying to place *in directo* certain previously formed combinations which seemed feasible, that the feat might be performed porismatically with 18 trees only, thus:

Fig. 3.

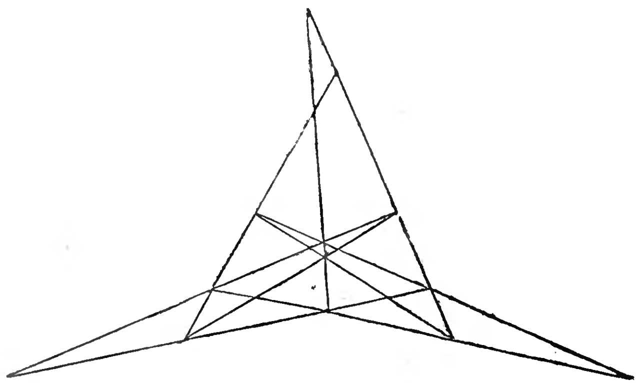


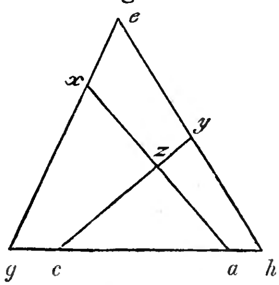
Fig. 3 is in fact fig. 1., with every line produced to meet every other line. A tree is to be supposed planted at every intersection.

Mr. Sylvester has investigated very curious practical methods (which I hope he will publish) of forming numerical combinations which shall possess certain assigned properties, and he has also long entertained the opinion that geometry, so far as it relates to position, is dependent on a higher or more

abstract science of form, just as geometrical magnitude is subject to the science of number. Though the tree problem had led me to consider combinatorial schemes for the arrangement of points, it was from conversation with Mr. Sylvester that I was induced to attribute any importance to the study of the symmetrical combinations which the notation of geometrical theorems of position presents, and to suppose it possible that, more generally, the study of functional forms might produce a method suggestive of new geometrical theorems. It would probably be well for the theories both of geometry and functions, if an expressive symbolic analogy could be contrived between geometrical and functional form. In the ordinary Cartesian equations of curves, the symbols of mere magnitude precede and almost wholly constitute the formulæ which, to a practised eye, disclose relations of curvature. I desiderate rather a converse system of notation, or at least a system in which more of the elements than + and — should directly symbolize form. I hail the introduction into the algebra of geometry of such things as spherical coordinates,—as elliptic sines and cosines,—as $r\ (\cos\theta + \sqrt{-1}\sin\theta)$ employed to denote a line of a certain length inclined to a given origin at a certain angle. Let us at all events pay more attention to the mere functional properties of the algebraic equations of curves, and to the geometrical interpretation of those properties, if we cannot invent a notation which shall prove a more direct and obvious index to the nature of their curvature.

I take this opportunity of publishing the following four theorems which occurred to me about fifteen years ago, and have never, to my knowledge, appeared in print; but I am little acquainted with the writings on the subject, and can scarcely expect that in a field so often gone over, any gleaning should be left. I will not at present further occupy the pages of this Magazine with a proof of them, but merely mention that the proof of them, like that of the preceding theorems, may be made to depend on the well-known constant property of the section of all transversals drawn across the same four straight lines diverging from a point.

Fig. 4.



1. If from two given points $(a\,.\,c)$ (fig. 4.) in the base $(g\,c\,a\,h)$ of a triangle (e, g, h), any two straight lines $(a z x, c z y)$ be drawn to the sides, so that the intercepts $(g\,x,\ h\,y)$ from the extremities of the base

may always bear to each other a given ratio, the locus of the intersection (z) of the lines so drawn will be a straight line.

2. If a m-lateral, all of whose sides but one pass respectively through given points, be inscribed in a given m-lateral, the remaining side of the former will always cut off from two fixed points respectively in the two sides circumscribing it of the latter segments which contain a constant rectangle.

Hence the problem—to inscribe in a m-lateral another m-lateral whose sides shall pass through given points—reduces itself to this; to draw through a given point a straight line which shall cut off from two given points, on two given straight lines, segments containing a given rectangle.

3. If a m-lateral, all of whose sides pass respectively through given points, have $m-1$ of its angles respectively on given straight lines, the locus of its remaining angle will be a conic section.

This theorem and its converse cases are very fertile. By assuming conditions which shall cause 3 points of the locus to lie *in directo*, we get, among others, the two theorems, which have formed the principal subject of the preceding paper.

By the angles of a m-lateral I understand here those angles only which are contained by the sides taken once round two by two in a pre-determined order, so that no side occurs more than twice. Otherwise a m-lateral has more angles than sides.

4. If the angles of a quadrilateral, and the intersection of one pair of its opposite sides lie respectively on 5 given straight lines diverging from a point, the locus of the intersection of the other pair of sides will be a 6th straight line diverging from the same point. This theorem admits of extension.

The pretty theorem of Dr. James Thomson of Glasgow, (*ante*, p. 41.) has suggested to me the following more general and fertile proposition.

5. If straight lines be drawn from the vertices of a triangle to any 2 points in its plane to meet the sides, the 6 points of meeting will be in a conic section. Hence, when 5 of the points lie in a circle, the 6th will lie in the same circle. If 3 of them lie *in directo*, so will the other 3. The intersections of the opposite sides of any hexagon of which they form the vertices, will lie *in directo*.

It is an interesting question whether it be not possible to contrive one or more schemes consisting of sets of 6 points, such that, if each set be supposed to lie in a separate conic section and all the sets but one be given, the remaining set shall porismatically follow. From 6 equations of the form

$$a + a_1 x + a_2 y + a_3 x^2 + a_4 xy + a_5 y^2 = 0,$$

we may obtain a symmetrical equation between any 6 pairs of coordinates (x, y), which shall be independent of the assumed constants. The *fixed symmetry* of such an equation will express that the six varying points corresponding to the varying pairs of coordinates lie in a conic section. I call it "the conic-sectional symmetry." To return to the question I have suggested as interesting, the following will be its algebraical statement. Is it possible to find a system of conic-sectionally symmetrical equations, such that no two of the equations shall involve the same 5 pairs of coordinates, and such that, from all of the equations but one, the remaining one shall be a necessary consequence? To solve this and similar problems, I want a condensed notation, as free as may be from accidental matter, of the conic-sectional and other fixed symmetries,—the essences, as it were, of genera of form. Rules of elimination between a given number of equations, all of a constant symmetry, may be stated as mere methods of combinatorial arrangement. In the few preceding sentences has been shadowed forth my idea as to a practical mode of establishing a connexion between the solution of geometrical problems of position, and the formation of arrangements of sets so as to possess assigned properties. Some brains may be better adapted for researches of the former kind; others, for those of the latter. Problems seeking one among finite combinations, when possible, may be worked out by plodding labourers, understanding, remembering, and applying rules, and knowing when to stop,—by machines, in fact, such as Mr. Babbage might construct. In such researches, however, *compendious* success, when it is due at all to reasoning, is generally due to reasoning of the inductive or analogical, not the syllogistic type. Such success, indeed, though helped out by reasoning, seems for the most part to spring from the luck of an appropriate inventive faculty akin to imagination.

XXV. *Notice on some peculiar Voltaic Arrangements. By* Dr. C. F. Schœnbein.*

I AM not aware that any philosopher has as yet pointed out the possibility of constructing voltaic arrangements which as to their mode of exciting current electricity must be considered in some respects at least as the very reverse of what our ordinary hydro-electric circles are. That such association can be made will appear from what I am going to state.

* Communicated by the Author.

Having of late made some researches with the view of discovering the true cause of the polar state which the electrodes acquire within electrolytic fluids during the passage of a current, I was led to observe a striking analogy between metallic peroxides on one side, and chlorine and bromine on the other, with regard to the electromotive power of those substances, all of them being highly what is called electro-negative. First let us consider the way in which the two substances last mentioned give rise to currents. If an open glass tube being tied up at one of its extremities with a piece of bladder and holding an aqueous solution either of chlorine or bromine be put within a tumbler filled with water, and both fluids connected by the means of platina-wires with a delicate galvanometer, a continuous current makes its appearance, the direction of which is such, as to indicate that the fluid holding chlorine dissolved, is to pure water what copper is to zinc. How is this current excited? Does it perhaps originate in some action exerted by chlorine upon platina? I do not know whether a weak solution of chlorine has at common temperatures the power to act chemically upon that metal; I strongly doubt it: but let us suppose such to be the case, it appears to me that the current in question cannot be accounted for in the manner alluded to. If the current was due to some chemical action of chlorine upon platina, its direction ought, according to the theoretical views of some distinguished electricians, (to De la Rive and Becquerel's for instance) to be the very opposite to that which is really observed, the chemical union of chlorine with any metal being in a voltaic point of view considered by them as equivalent to the oxidation of metallic bodies. But it is much less the direction of the current in question which makes me think that it (the current) cannot be excited in consequence of the formation of some chloride of platina, than the fact that any current should be called forth under the circumstances mentioned. Agreeably to my views, and I think they will coincide with those of Faraday, no current at all would be excited within the arrangement spoken of if no electrolytic body were decomposed there, though chlorine should happen to act ever so powerfully upon platina; but if the current in question is not due to any action of chlorine upon the metal mentioned, we may ask, what is the chemical cause that disturbs the electrical equilibrium within our arrangements? Though I am not aware that chemists allow chlorine or bromine to decompose water in any sensible degree without being assisted by the agency of light or heat, I am nevertheless prepared to think that such an action takes place, and that it is this which must be considered as the chemical cause of the current

spoken of. According to my opinion, chlorine or bromine bears a similar relation to the hydrogen of water that distilled or amalgamated zinc does to the oxygen of (acidulated) water: in either case there is no apparent chemical action, *i. e.* no decomposition of water previously to a circuit being formed or a path opened for a current. But no sooner has the circuit been completed in our two cases, than both the decomposition of water, and the formation either of muriatic acid or oxide of zinc, begins to take place, and a current makes its appearance. So far the two cases appear entirely similar to each other, but there is some essential difference between them which I will point out. In one case, chlorine is hydrogenized at the expense of the hydrogen of water, in the other zinc is oxidized at the expense of the oxygen of water; in one case oxygen is set free at the chlorine, in the other hydrogen is evolved at the zinc; or what comes to the same, in one case hydrogen is transferred from water to chlorine, in the other case oxygen is carried over from water to zinc. Now it is very easy to conceive that chemical actions being so diametrically opposite to each other as those in question obviously are, cannot produce the same electro-motive effect, in other terms cannot cause the electrolyzation of water in the same manner. Let us consider in what way water is electrolyzed in each of the cases under discussion. The particle of oxygen separated from one molecule of water by a particle of chlorine, unites with the hydrogen contained in the molecule of water which is contiguous to the oxygen disengaged, the oxygen of the latter molecule of water combines with the hydrogen of a third one, and so on, until the whole row of contiguous particles of water placed between the electrodes (the ends of the wire connecting the two fluids) have undergone a similar decomposition and recomposition, and oxygen is at last disengaged at the positive electrode. The atom of hydrogen eliminated by the action of zinc from the molecule of water which is in immediate contact with that metal associates itself with the oxygen of a third molecule, and so on, until an atom of hydrogen is evolved at the negative electrode. From what I have just now stated it appears that the particles of oxygen contained in the molecules of water approach zinc whilst they recede from chlorine, or that the atoms of hydrogen move towards chlorine whilst they recede from zinc. If it be admitted that a current passing through an electrolytic body is made up by or is identical with a succession of chemical decompositions and recompositions which the molecules of the electrolyte undergo, we can easily conceive that what we term the direction of a current is determined by the direc-

tion in which one of the constituent parts of the electrolyte moves with regard to the substance which chemically acts upon the electrolyte. If the electrolyte be water for instance, and its hydrogen happens to recede from the body that operates the composition of water, the current goes along with the hydrogen and we call the body causing the chemical action electro-positive. If, on the contrary, the oxygen of the electrolyte recedes from the substance by means of which chemical action is brought about, the current approaches that substance, and we term the latter electro-negative. Generally speaking, the direction in which the atoms of hydrogen move during the electrolyzation of water is also the direction in which the current circulates, both motions being one and the same thing. After what has been said it cannot surprise us that the chemical decomposition of water caused by chlorine or bromine produces a voltaic effect exactly opposite to that which is obtained by the decomposition of the same electrolyte brought about by the action of zinc. As to the peroxides, it cannot be doubted that their surplus of oxygen acts the same voltaic part with regard to water, as chlorine and bromine do in the cases which have just now been discussed; but how does it happen, that oxygen, being in a combined state, exerts any tendency to unite with the hydrogen that is chemically associated with oxygen? I tried to answer this puzzling question some time ago in a letter to the editor of the Phil. Mag.: having however ascertained since some facts which do not well agree with my views developed in the paper alluded to, I am inclined to consider the latter as erroneous. As I think it to be a fundamental principle of the theory of galvanism, that any hydroelectric current, even the weakest one, is absolutely dependent upon real chemical action, in other terms, that hydroelectric currents and certain chemical actions are the same things, I am forced to admit that the decomposition of peroxide of lead for instance, which takes place when that substance constitutes a voltaic circle with (acidulated) water and platina, is an immediate action; that is to say, the immediate effect of the tendency of one proportion of oxygen contained in the peroxide of lead to unite with the hydrogen of water, is just as immediate as the tendency of zinc to combine chemically with the oxygen of water. But such a supposition certainly implies something which seems to be altogether impossible, or at least highly improbable, namely that different portions of oxygen being in different states of combination, are not to be considered as being absolutely identical with each other with regard to their voltaic action or properties.

A supposition of this kind sounds indeed very strangely, and

I am afraid will not be much relished by the majority of chemists, so much the less that oxygen being in a free state seems to have no voltaic action at all upon water*. Extraordinary as the matter in question may appear to be at the first sight, it appears less so, in my opinion at least, on a closer inspection of the case. Dumas has shown, and as far as I can understand the subject, in a very satisfactory manner, that chlorine is capable of losing altogether its ordinary chemical bearings in replacing the hydrogen which is contained in acetic acid. In chlor-acetic acid the element named acts the part of hydrogen so perfectly well, that the compound last mentioned has all the essential properties of common acetic acid; and chlorine is so much masked in chlor-acetic acid, that by means even of the most delicate tests its presence there cannot be made perceptible. Now if chlorine, being contained in different compounds, can exist in states which in a chemical point of view must be considered as being diametrically opposite to each other, if chlorine be apt to lose its ordinary affinities and to acquire new ones, I ask why should oxygen, and other bodies also, not be capable of undergoing a similar change of chemical character when they are caused to enter into certain combinations; and why should it be beyond the reach of possibility that the second atom of oxygen contained in the peroxides, differs as to its chemical functions from the oxygen contained in water, and that it is possessed of properties which make it to a certain degree similar to free chlorine, bromine, iodine, &c. with regard to its action upon the hydrogen of water? In support of the view that the same substance is capable of performing very different chemical functions, I cannot omit to allude to the beautiful and interesting researches of Professor Graham on the constitution of salts. That distinguished chemist has rendered it highly probable, if not altogether certain, that water acts in the very same compound very different parts; and he has demonstrated besides, that substances differing in many respects very widely from that fluid may replace it, and take upon themselves its different functions. These phænomena of metalepsy, highly interesting as they are in a purely chemical point of view, appear to me to be still more so in a voltaic one, because they promise to offer the key to the comprehension of many voltaic facts which up to this present moment bear an anomalous character. Though

* A solution of oxygen in (acidulated) water being separated by a piece of bladder from (acidulated) water containing no oxygen dissolved, did not cause an obvious deviation of the needle when both fluids were connected with a most delicate galvanometer.

we may take it as a certain fact that one portion of the oxygen of the peroxides, that chlorine, and bromine are bodies which act in the same manner upon water, we do not know for all that the way in which they excite a current; or rather we do not know why any of the three substances first named acquire, under certain circumstances, the power of decomposing water. According to the views of De la Rive and some other electricians, the cause of the currents produced by hydro-electric circles is chemical action. Convinced as I am of the general truth of the principle laid down by the philosopher of Geneva, I cannot help doubting the correctness of that doctrine of his according to which chemical action must always precede the appearance of a current. As far as I understand the chemical theory of galvanism alluded to, it cannot account for the disturbance of the electric equilibrium which takes place within a voltaic circle formed out of such substances as do not chemically act upon each other under ordinary circumstances. Amalgamated or distilled zinc not being attacked by acidulated water, peroxide of lead not being acted upon by nitric acid, bromine or chlorine not decomposing water, ought not, according to De la Rive's hypothesis, to excite any current if arranged so as to constitute with platina a voltaic circle. In asserting that the theory in question cannot account for the currents which are excited by the last-mentioned arrangements, I am well aware of what Mr. De la Rive has brought forward with the intention of vindicating the correctness of his views. He maintains, that in the cases above stated there is some feeble chemical action taking place, and that it is this action which must be considered as the cause of the initial current. This current being the effect of a chemical action, becomes, according to the opinion of De la Rive, itself the cause of an additional current by effecting electrolyzation, &c. (*Récherches sur la cause de l'Electricité Voltaique*, 1836, *p.* 128.) Ingenious as these views may be, I cannot convince myself of their truth until Mr. De la Rive has given proofs more decisive than those are which he has hitherto offered in favour of his assertion, made either explicitly or implicitly, that it is only a film of hydrogen which protects amalgamated zinc against the action of acidulated water; that peroxide of lead and nitric acid do chemically act upon each other; that chlorine is capable of decomposing water in darkness, &c. But if no such chemical actions take place in the cases mentioned, and if nevertheless chemical action be the real cause of hydro-electric currents, how are we to account for the excited currents in question? We know as yet very little indeed of the na-

ture of that force which causes what we call chemical actions, and we are far from being acquainted with all the circumstances and conditions which bodies must be placed in, in order to make them affect each other chemically. On account of that very ignorance of ours, it appears to me to be no hazardous or inadmissible conjecture if we suppose the possibility of certain compounds and single bodies acting chemically upon each other only when arranged together in a particular manner as to their juxta-position. Peroxide of lead, aqueous nitric acid, and platina are bodies which, under ordinary circumstances, are perfectly indifferent to each other; but you have no sooner arranged them into what we term a voltaic circle, than the peroxide is decomposed, nitrate of lead produced, and water electrolyzed; that is to say, the hydrogen of the latter compound caused to act upon the surplus oxygen of the peroxide: chlorine, water, and platina on one side; zinc, acidulated water, and platina on the other, are similarly circumstanced, no chemical reaction taking place between them unless put together so as to form a circle. I know well enough that the phænomena in question are interpreted by the voltaists in favour of the fundamental principle of their theory, and that the chemical actions just spoken of are considered by many philosophers as effects of a current produced by mere contact. But there are many facts known in our days which, in my humble opinion at least, cannot be reconciled with the theory alluded to, and which prove that a current excited within an hydro-electric arrangement is entirely dependent upon the chemical action taking place there. I consider as one of these facts the relation which exists between the passage of a current through an electrolyte, and the decomposition of the latter. Dr. Faraday's beautiful researches have thrown a strong light on the mutual dependence of the two phænomena; that celebrated philosopher does however maintain that feeble currents can pass through electrolytic bodies without causing decomposition. I ascertained some time ago the important fact, that electrolyzation and the passing of a current are so absolutely dependent on each other, that not even the weakest current can go through any electrolyte without decomposing the latter*. Now, if such be the case, are we not allowed to conclude, at least to con-

* I perceive from No. 20 of the *Comptes Rendus*, that Mr. W. R. Grove has made a communication to the French Academy, in which there is stated the same fact with regard to water. In giving it, as a novel one, Mr. Grove was most likely not aware of what I published six months ago in the *Bibliothèque Univ.* No. 35, p. 189, and in the *Lond. & Edin. Phil. Mag.* No. 85, p. 45.

jecture, that electrolyzation, i. e. chemical action of a specific kind and voltaic electricity are the same thing; in other terms, that what we call an hydro-electric current is but a particular motion of the elementary molecules of an electrolyte? There is another fact which, in my opinion, bears strongly upon the subject in question. As far as my knowledge goes, a *conditio, sine qua non,* for constructing an efficacious hydro-electric arrangement, is, that one of its component parts be a chemical compound of a certain kind, i. e. an electrolyte. Placing myself upon the ground occupied by the strict voltaists, I cannot see the necessity of such a condition; and I am unable to conceive why simple bodies of themselves (without the interference of electrolytes) should not be capable of producing currents. But if I adopt the principles of the chemical theory of galvanism, and if besides I consider the hydro-electric current as a particular chemical motion of the elementary molecules of the electrolytic compound, I can understand well enough why an electrolyte constitutes an indispensable part of an hydro-electric arrangement, and why for instance a pair of zinc and platina being plunged into mercury is incapable of exciting current electricity, although the latter metal chemically acts upon zinc, and though the three bodies mentioned are arranged after the manner of a voltaic circle. According to the views which I now hold, and to the adoption of which I have been led by my late researches, I am very far from being prepared to allow (as I formerly did in accordance with De la Rive) to any sort of chemical action the power of causing the phænomena of a current. It is only the decomposition of an electrolyte, which I consider as the source of hydro-electric currents, and it is for that reason why I do not believe that nitric acid, for instance, produces any current within a voltaic arrangement by acting chemically upon some metallic parts of the latter. I hope to be able to publish before long a series of results, obtained from researches which I recently made for the purpose, to prove the correctness of my views. For the present I deem it sufficient to state one fact in support of them. Two cups are filled, one with chemically pure nitric acid of ordinary strength, the other with an aqueous solution of potash, (being entirely free from air) and the two cups connected with one another by means of a platina wire. If now a copper wire be put in the acid fluid, a zinc wire in the alkaline solution, and the free ends of both wires made to communicate with the galvanometer, a current makes its appearance, the direction of which is such as to show that zinc is positive to copper. The same result is always obtained

whatever may be the degree of dilution of either fluid made use of. As far as I know, chemists do not allow pure zinc to be in the least chemically affected by an aqueous solution of chemically pure potash, whilst copper is readily dissolved by nitric acid. Now according to the views of De la Rive, the copper wire in the arrangement before mentioned ought to be positive with regard to the zinc, which is immersed in the alkaline fluid. The philosopher of Geneva has tried to reconcile some similar facts disagreeing with this theory by asserting, that the largest portion of electricities separated from each other by the action of nitric acid upon some metal reunites upon the surface of the latter, on account of the great conductive force of the nitric acid (see the above-mentioned Memoir, p. 39.). But it appears to me that such an explication cannot be applied at all to the case in question, because the alkaline solution and zinc of themselves (without making part of a closed circuit) do not chemically act upon each other, consequently cannot (according to the notions of De la Rive) produce any current, and because that metal continues to be positive to copper, though the acid into which the latter (the copper) plunges be ever so much diluted with water, that is to say, its conductive force ever so much diminished and brought even below that of the alkaline solution. Before I leave the subject in question, I cannot omit in a general way to state that the result of my recent researches seem to prove that the voltaic relation which any two metals bear one to another within pure water is not changed at all, if the latter substance be mixed in any proportion with sulphuric, nitric, muriatic acid, potash, ammonia, &c. and that portions of the same metal, though they are plunged into very different aqueous fluids and are very differently acted upon by the latter, cannot assume opposite voltaic conditions. For instance, if copper be negative to tin within water, these metals continue to preserve that voltaic relation to one another whether they be placed within acidulated water or within aqueous ammonia. If copper be negative to lead within water, this voltaic bearing is not changed by putting the metals mentioned either into strong or into much diluted nitric acid. If one piece of zinc be plunged into a solution of potash, another in water mixed with sulphuric acid, muriatic acid, &c., the two pieces of zinc do not give rise to any current. I am well aware that these assertions are strongly at variance with the results of Davy, De la Rive, and others, and indeed also with what very simple experiments seem to show; but I am prepared to say that if the same two metals are sometimes positive, sometimes negative with regard to each other, accord-

ing to the chemical nature of the fluid into which they are plunged, this change of voltaic character is only apparent and not real, and that it is dependent upon some secondary circumstances which occasion a modification of the surfaces of the metals employed. Having above ventured to start the idea, that many substances, being under ordinary circumstances altogether chemically indifferent to each other, might become capable of acting upon one another when arranged together in a peculiar manner, and presuming that such a conjecture will be considered by the majority of chemical philosophers as very extravagant and wild, I am desirous to say a few words more on the subject. We know now-a-days a series of chemical phænomena called forth not by what we term "affinity," but by the mere presence of certain bodies, by what the French call "action de présence," Berzelius, "force catalytique." The decomposition of peroxide of hydrogen caused by the precious metals and by their oxides; the transformation of alcohol into acetal, aldehyde, and acetic acid brought about by the joint action of platina and oxygen; the well-known union of oxygen with hydrogen effected by platina*, are instances of chemical actions occasioned by a force which is altogether unknown to us, and widely different from what we conceive common affinity to be. Now if a substance can cause either the union of bodies with one another, or the decomposition of compounds, without entering into any combination with them, why, I ask, should the case be impossible, that certain substances do only chemically act upon each other, in consequence of their being put in contact with one other in a peculiar way? For myself, I do not see any reason why such a thing should be impossible; at any rate, the fact that in many instances bodies do chemically act upon each other, merely as far as they are arranged in the shape of a circuit, cannot be denied any longer. The question is only how the fact is to be interpreted. Now if we consider the current excited in such cases as being due to chemical action, it seems to me that we cannot avoid arriving at a conclusion like that I have come to.

Bâle, July, 1839.

* I have reasons to doubt the correctness of the very ingenious account which Dr. Faraday has given of the phænomena in question, and shall not be long in making them known. I also consider the view taken by Mr. De la Rive on the subject as erroneous.

XXVI. *On the Equivalents of the Cambrian and Silurian Systems in Belgium; embodied in a "Report on the Progress of the Geological Map of Belgium, during the year* 1838." *By* A. H. Dumont, *Member of the Royal Academy of Sciences of Brussels.**

THE ancient tracts of Belgium are beyond contradiction the most important, whether viewed in a scientific light, that is to say, as regards the divisions which mineralogy and palæontology enable us to establish, or commercially, on account of the riches in combustibles and metalliferous ores which they contain.

From these considerations the Academy will doubtless find that it was desirable not only to make known the different modifications which these tracts present, but also their relations to the other analogous districts in neighbouring countries.

In 1837 and 1838, I called the attention of the Academy to the agreement which exists between the formations of the Eifel and the two lower systems of the anthraxiferous districts of Belgium.

This year I have endeavoured to make the same comparisons between our systems and those which Mr. Murchison has recently established in Great Britain.

For this purpose, I undertook, along with our learned colleague M. d'Omalius d'Halloy, and M. de Verneuil, a distinguished palæontologist, a special journey into Wales, and I think that I have succeeded in establishing a complete parallelism between the English formations and our own.

The west of Great Britain completely reminds us of the south-east of Belgium. We find there the cultivated hills of Condros, and the barren plains of the Ardennes, which agree in the composition of their rocks with the divisions established by Messrs. Murchison and Sedgwick, under the name of the Silurian and Cambrian systems, corresponding exactly to those which M. d'Omalius designated, in 1808, by the names of slaty and anthraxiferous (*ardoisier et anthraxifère*).

If we survey the Cambrian district, it is easy to recognise two systems there, distinct, as much by their relative position as by their mineralogical composition. The first which I observed amongst others in the neighbourhood of Dinas Mowddwy, forms the central mass of Wales, and presents a number of rocks which may be considered as true slate, and which in a great many places are worked. This mass an-

* From the *Bulletins de l'Académie Royale des Sciences, &c. de Bruxelles.* Vol. V. p. 634.

swers to the middle system, which I established in 1836, in the slate district of Belgium.

Our excursions did not allow us to observe whether the lower slate system exists in Wales, which, moreover, must be difficult to ascertain, on account of the changes which must have been produced by the porphyroïd rocks of Snowdon and Cader Idris.

To the east of the middle Cambrian district, there is an assemblage of rocks quite analogous to that which forms our upper slate system in Belgium, which may be proved by traversing the mass situated between Mallwyd and Garthbibio.

Further eastward successively appear superposed with remarkable simplicity, the different divisions which Mr. Murchison has distinguished in the Silurian system, and which correspond to the three lower divisions of the anthraxiferous system.

As I should deviate from the purpose of this report by giving in detail the observations which I had an opportunity of making in Wales, I shall limit myself to pointing out the correspondence between the different members of the Silurian and anthraxiferous formations, in the following table:

TERRAIN HOUILLIER.			Coal-measure. Millstone grit.		
TERRAIN ANTHRAXIFÈRE.	*Système calcareux supérieur.*	*Calcaire.* *Dolomie.* *Calcaire.*	Carboniferous Limestone.		
			Old Red Sandstone.		
	Système quarzo-schisteux supérieur.	*Psammite.*	Upper Ludlow rock.	Ludlow formation.	SILURIAN SYSTEM.
		Calcaire subordonné	Aymestry Limestone.		
		Schiste	Lower Ludlow rock.		
		Calcaire subordonné	....		
	Système calcareux inférieur.	*Calcaire.* *Dolomie* *Calcaire.*	Wenlock Limestne.	Wenlock formation.	
	Système quarzo-schisteux inférieur.	*Schist gris fossilifère*	Wenlock Shale.		
		Schiste et psammite rouge, poudingue.		Caradoc & Llandeilo formation.	
		Psammite, quarzite, schiste.			
TERRAIN ARDOISIER.	*Supérieur*			CAMBRIAN SYSTEM.	
	Moyen				
	Inférieur				

On the mere view of this table we are struck with the agreement which there is between the two districts; and in fact, all the Belgic divisions recur in England in perfect succession. There is however a difference in their general composition which it may be useful to mention; it consists chiefly in the somewhat rudimentary state to which the calcareous rocks are reduced. Thus, whilst in the Eifel and the southern part of the provinces of Hainault, of Namur, and Liege, the lower calcareous deposit acquires an enormous developement, the

Wenlock limestone is found to occupy but a very small space, to narrow progressively towards the south-west, so as to be reduced near Ludlow, to a breadth of six miles, and further on to lose itself in the schist.

But if the calcareous rocks are much more developed in Belgium than in Great Britain, the schistose and psammitic rocks are on the other hand much more prevalent in the latter country.

I have yet to speak of a very considerable deposit, which prevails principally in the counties of Hereford and Brecon, and to which the name of old red sandstone has been given. It is doubtful whether this formation has a representative in Belgium. If it be wanting, its place is marked between the upper quartzo-schistous system and the upper calcareous system. If it exist, we must consider it as an extraordinary development of the upper psammitic part of the quartzo-schistous system.

The English divisions established by Mr. Murchison are founded upon the existence of fossils which appear to be different in each of them, and are consequently very proper to characterize them. But these divisions, which are very good for England, must present palæontological differences more or less remarkable in other countries, and this is in fact what takes place in Belgium. We shall draw the attention of the Academy to this at a future period.

The Academy will moreover learn with pleasure, that we have also recognised our four anthraxiferous systems in the ancient tracts of the Boulonnais. In fact, if we direct our course from the coal district of Ferques towards Landrethun, we see the following rocks appear successively, dipping under the coal formation:

1. Limestone.
2. Dolomite.
3. Yellow psammite.
4. Limestone (Steinkalk).
5. Dolomite.
6. Limestone.
7. Schist and red psammite.
8. Grayish schist.

The limestone and the dolomite, Nos. 1 and 2, belong to the upper calcareous system, the fossils of which moreover it contains.

The psammite, No. 3, has the characters and the position of the upper quartzo-schistous system.

The limestone and the dolomite, Nos. 4, 5 and 6, represent the three divisions of the lower calcareous system, and contain the fossils characteristic of it; for example, the *Cyathophyllum quadrigeminum*, the *Calamopora polymorpha*, the *Terebratula concentrica*, some Spiriferæ and Euomphali, &c.

I ought however to observe, that we also found there Productus, and other shells which are generally met with in the upper calcareous system.

The schist and the red psammite, No. 7, occupy the position of the lower quartzo-schistous system, of which it has moreover all the mineralogical characters.

Lastly, the grayish schist, No. 8, may be considered as belonging to the upper part of the slate district.

Such are the approximations which my foreign journeys have enabled me to make; we will now review the observations I have made in our own country.

I commenced by determining the boundaries of the slate and anthraxiferous districts of Brabant, and of the northern part of the province of Hainault, which led me to ascertain that at the farm of Hongrée, near Ronquières, the conglomerates, the schist, and the red psammites of the lower quartzo-schistous system repose in unconformable stratification upon the truncated beds of the slate formation. Now it is to be remarked, that in the north of Belgium the upper slate system is generally very little developed or entirely wanting, which explains this fact which is contrary to what I have hitherto observed on the northern edge of the Ardennes, where the slate and anthraxiferous formations are not merely stratified in a perfectly conformable manner, but also pass so insensibly from one to the other, that it is impossible to trace a line of demarcation between the two formations.

I also discovered several undescribed beds of porphyry, namely, at a quarter of a league north of Henamyères, to the south of Audimont, to the south and south-west of Fauqué, in the road from Monstreux to Grand Abay, and near the road from Monstreux to Nivelles. These beds hardly deviate from a straight line drawn from Enghien to Nivelles.

The porphyries vary in texture, and have produced some slight alterations in the schistose rocks near Monstreux, which will be noticed in detail, when we have examined the slate formation of the north of Belgium in all its extent.

I afterwards paid particular attention to the tertiary and modern chalk formations. I have made a great many excursions in the provinces of Flanders, Antwerp, and Limburg, in order to have a complete idea of the different modifications which they present; and I have thus laid down the outlines of operations, the details of which only remain to be filled up. To the latter also I have already applied myself in the district of Fauquemont (a province of Limburg), and I hope that the five northern provinces of Belgium will be finished next year. The district of Fauquemont is one of the most inter-

esting, on account of the variety of the rocks, the modifications which they have undergone, and the dislocations of the strata.

The forest of Aix is composed of sand and of freestone belonging to the lower chalk formation; but in advancing towards the N.W. there successively appear chalk, the limestone of Maestricht, and even the lower tertiary sand.

Above these rocks a bed of the upper tertiary formation extends, its base being composed of flints and its upper part of alluvium. We may easily be satisfied that this layer originally covered the whole country, and would still conceal the subjacent rocks, but for the numerous valleys which furrow it, and whose origin is consequently posterior to the last tertiary deposit. Now, all these valleys owe their origin to faults and to transverse fissures connected with them. At one time there has been a simple change of level among the masses separated by faults, and at another time there has been an unequal movement [*mouvement de bascule*] having for its axis the line of a fault, which has deranged the primitive horizontality of the strata.

For instance, the mass included between the fault of the valley of the Guele, and that which caused the valley in which the rivulet Nysweiller flows, has been raised in comparison with the mass situated to the north of this latter fault, to such a degree, that the sand of the forest of Aix occupies a higher level than the chalk which stretches from Aix-la-Chapelle to Wittem.

The isolated mountain of Louisberg to the north of Aix-la-Chapelle, must likewise have been raised like a plug, because the chalk at the summit is at a higher level than that which prevails to the west of the city.

Besides the great faults which have divided the formation into large masses and changed their levels, there is an infinity of small faults which have subdivided these masses and produced new dislocations; so we must nearly always expect to find a disagreement between the level of beds of the same kind, at the two sides of a valley, or to see on one side of the valley beds of a kind different from those which had be enobserved on the other side.

These observations entirely confirm what we advanced on thesubject of faults in our preceding report.

In traversing the district included between Aix-la-Chapelle and Kunraad, the chalk is found to lose its earthy texture, and becomes so compact that it resembles certain Jura limestones. The Maestricht limestone also acquires a texture and an aspect which makes it so difficult to recognise, that it is

often no longer possible to distinguish it from a modified chalk.

These modifications being observed along a line running from N.W. to the S.E., and terminating at Borcette, we may conclude with great probability that the changes which the cretaceous rocks have undergone were the effect of some plutonian agencies connected with the phænomena of the mineral and hot springs of this town.

I shall conclude this report with some remarks on the modern deposit of Flanders.

It consists for the most part of a clay more or less plastic, grayish, calcariferous, sometimes sandy, and containing products of art, and some shells analogous to those which live upon our coasts at the present period. This clay forms a horizontal bed which in some localities is three metres thick.

Under the clay in a great many places is found a layer of turf, which sometimes attains the thickness of five metres, and which is composed, according to the observations of M. Belpaire (Memoir on the changes which the coast from Antwerp to Boulogne has undergone), of two distinct parts, namely, the upper part of terrestrial, and the lower of aquatic plants. Beneath the turf clay is also sometimes found, but more generally there is nothing but sand, in which shells analogous to those now found in the sea are met with.

The modern deposit forms a band bounded on the sea side by the Dunes, and on the land side by a sinuous line traced on the map which accompanies the Report*. It is easy to distinguish this deposit from the neighbouring formations by its clayey nature, the fossils it contains, and its perfect horizontality; it is besides covered with rich pastures through nearly the whole of its extent, which also distinguishes it from the arid sandy soil that surrounds it. The connexion which exists between this deposit and the clay of the *Polders*, which are still actually forming, and the presence of shells similar to those which live on the coasts, sufficiently indicate its recent origin, and that the sea, at a period not very remote, advanced into the lands nearly as far as Dixmude, Ghistel, Bruges, Assenede, &c. as may be seen upon the map.

Our honourable colleague, M. Belpaire, from the investigation of historical documents, comes to the same result. He even fixes the origin of the clay formation at the period of the

* This refers to a small map accompanying the Report, as given in the *Bulletin*, showing the geographical extent of the modern deposits of Flanders, and the maritime limits of the ancient Belgium: the former appears as an edge or border, extending from the ancient to the present coast-line, and in the direction from Furnes to Antwerp.

Roman dominion. Now it is exceedingly interesting to see history and geology uniting for the purpose of solving questions relative to recent formations*.

XXVII. *On the Polarization of Light by Living Animals.* By J. F. Goddard, *Lecturer on Optics, &c. at the Royal Gallery of Practical Science.*

To the Editors of the Philosophical Magazine and Journal of Science.

Gentlemen,

On repeating the experiments published in 1816, and others subsequently noticed by Sir D. Brewster, on the polarizing property in the eyes of fishes and other animal substances, with my Polariscope, after observing that the *scarf skin* of the *human subject*, sections of *human teeth*, the *finger nails*, *bones of fishes*, &c. possessed the same property, I was led to examine some living objects, when I discovered that, among many others, the larvæ and pupa of a tipulidan gnat (the *Corethra plumicornis*) possessed this property also, and that in a very eminent degree. Its existence in the different substances above enumerated is exceedingly interesting and important; but that it should also exist in living animals is infinitely more so, and opens a new field altogether, disclosing characters that lead to an intimate knowledge of their anatomy, and which cannot possibly be discovered by any other means.

This creature is found in large clear ponds, generally in great abundance when met with, but this is by no means common. Having constructed a water trough, made with two slips of glass about 1·25 inch wide and two inches long, with very narrow slips of thin glass cemented with Canada balsam between them, at the bottom and sides, thus leaving it open at one end with about the 0·050 of an inch space between in the middle, I filled it with clear water in which I placed some of the larvæ; and such is the extraordinary transparency of the creature, as to display in a most beautiful manner the whole of their internal structure and organization, and which, when viewed in polarized light, presents the most splendid appearances. Thus when they place them-

* The papers of this kind on the æstuary of the Yare by Messrs. J. W. Robberds, and R. C. Taylor, Phil. Mag. & Annals, N.S. vols. i. ii. 1827, and those of Mr. Lyell, and Mr. Babbage, &c. have lately become the subject of discussion in the *Discorso* of Sig. D. Paoli, "Del Sollevamento e del Avvallamento di alcuni Terreni:" Pesaro, 1838.—Ed.

selves with their head and tail both in the plane of primitive polarization, or in a plane at right angles to it, they have no action upon the light transmitted through them; but when in a plane inclined 45° to the plane of polarization, the light is depolarized, their whole bodies becoming illuminated in the most brilliant manner, varying in intensity according to their size, and the nature of the different parts and substances; the peculiar interlacing of the muscles marking out regular divisions, which, as the creature changes its position with regard to the plane of polarization, exhibit all the varied hues and brilliant tints that have rendered this important branch of physical optics so exceedingly interesting.

And while thus viewing them, if we place behind a thin plate of sulphate of lime or mica, the change and play of colours as the creature moves are greatly increased, and are exceedingly beautiful.

These phænomena in the larvæ of the *Corethra plumicornis* are seen if possible in a more splendid manner in the spawn of many large fishes, but more particularly in the young fishes themselves, which in their early state are, many of them, equally transparent, particularly those of marine production.

The first time I showed them was on the 7th June to Mr. F. Watkins, afterwards to T. E. Wilks, Esq. and other gentlemen; and on Tuesday evening the 25th June, I read some notes on the subject, and exhibited them with many other experiments, to the members of the Zoological Society, at one of their regular meetings.

J. F. Goddard.

XXVIII. *Intelligence and Miscellaneous Articles.*

ANTARCTIC EXPEDITION.—TERRESTRIAL MAGNETISM.

THE preparations for the Expedition to the Antarctic Regions under the command of Capt. Jas. Ross, are in great forwardness; and the instructions, drawn up with a view to render it of the greatest service to every branch of science, have occupied the attention of all the committees of the Royal Society.

Observations on the phænomena of terrestrial magnetism being among the principal objects of the voyage, a translation of Professor Gauss's General Theory, which has been made for the furtherance of this object under the direction of Sir J. Herschel, will appear next week in the Sixth Part of the Scientific Memoirs, in addition to other papers on the same subject by Gauss and Weber, which have been prepared under the inspection of Major Sabine and Professor Lloyd.

SEPARATION OF MAGNESIA FROM LIME AND ALUMINA.

According to M. Anthon, when tungstate of soda is added to a solution containing the above-mentioned earths, the lime and alumina are precipitated together in the state of tungstates, even should the solution be rather acid: the magnesia is left in solution, and tartaric acid prevents the precipitation of the alumina. Tungstic acid cannot be substituted for the tungstate of soda.

Journal de Chem. Med. June 1839.

UREA: MODE IN WHICH IT EXISTS IN URINE.

According to MM. Cap and Henry, Urea does not exist in urine uncombined, but united with different acids in different beings —in man combined principally with lactic acid; in ruminating animals with hippuric acid; in serpents and birds with lithic acid, or at least with the peculiar acid which according to M. Liebig is its radical.

The natural lactate of urea obtained from the urine of man is identical with the same salt artificially prepared; the salts of urea are easily obtained by double decomposition.

Journal de Pharm. Mars 1839.

LACTATE OF UREA.

According to MM. Cap and Henry, when urine is evaporated, there is deposited a considerable quantity of crystalline salts, formed principally of chlorides, alkaline sulphates, calcareous and ammoniaco-magnesian phosphates. If this saline mass be separated by filtration, a brown very acid liquor is obtained, on the addition of alcohol to which, crystalline and hygrometric grains are formed, which when purified by charcoal become prismatic deliquescent crystals, of a sharp taste and which redden litmus paper. If these crystals are treated with hydrate of zinc and with alcohol, there is obtained a lactate of zinc, which is insoluble in the alcoholic liquid, but soluble in water; and also by evaporation of the alcohol very pure urea, which is not hygrometric, and possessed of all the characteristic properties of that substance. It is then evident that the crystalline grains obtained by the simple concentration of the urine freed from the chlorides and other salts, are principally constituted of lactate of urea.

Artificial lactate of urea is obtained by first preparing lactate of lime by adding slacked lime or chalk to sour milk; this salt is then decomposed by dissolving 100 parts of it which have been dried at 248°, in 200 parts of hot water and 41 parts of crystals of oxalic acid; this solution contains 75 parts of lactic acid as contained in salts, and there are to be added to it 73 parts of pure dry urea; the solution is to be filtered, evaporated with a gentle heat, and set to crystallize.

It is however preferable to prepare lactate of urea by double decomposition, either by treating the oxalate of urea with lactate of

lime, or by sulphate of urea and lactate of oarytes. The oxalate of urea has been described by Berzelius; it is composed of 37·44 oxalic acid + 62·56 urea; it is prepared by direct combination; by solution in water and evaporating with care, the salt is obtained either in a mass of interlaced prismatic needles, or in pearly laminæ.

The natural and artificial lactate of urea crystallize in elongated six-sided prisms, the summits of which are inclined. These crystals are white, hygrometric, and very deliquescent; their taste is cooling and sharp; they are very soluble in water and in alcohol and ætherized alcohol, but much less so in pure æther. When moderately heated they first fuse, then volatilize without decomposition and sublime. If the heat be too great the salt decomposes, leaving a black coaly residue.

Lactate of urea is formed of

Lactic acid	50·39
Urea	49·61—100
Or Lactic acid (anhydrous) . . .	1 equivalent
Urea	1 ditto
Water	1 ditto

Journal de Pharm. Mars 1839.

SUBSESQUIACETATE OF LEAD. BY F. WOHLER.

Matteucci has accurately observed the manner in which acetate of lead is affected at a high temperature, but he has not explained it, for he was unacquainted with the true composition of acetic spirit. The decomposition is in fact very simple, and affords another instance of the ready explanation which might be given of the decomposition of organic compounds at high temperatures, if it were possible in every case to expose them to well-determined degrees of heat uniformly throughout their mass.

If anhydrous acetate of lead be exposed in a glass vessel to a uniform temperature, it fuses, according to Matteucci, at 536°, into a transparent liquid, which boils in a uniform manner at a little higher temperature: the ebullition is due to the formation of carbonic acid and acetic spirit, which are given out; and the latter may be condensed by a long refrigerating tube. At last a period arrives at which the salt suddenly loses its liquid state and becomes a porous white mass; this is the subsesquiacetate of lead; it dissolves readily in water, carbonate of lead separating, which is formed as a secondary product in small quantity. The dense solution, evaporated to the consistence of a syrup out of contact of the air, deposits after some time the salt in pearly crystals; they are laminated and grouped in a concentric manner.

This decomposition of the neutral acetate of lead consists then in losing one third of its acetic acid at 536°, which is converted into carbonic acid and acetic spirit, while the remaining two-thirds combine with all the oxide to form a subsesquisalt, which is not decomposable at this temperature.—*Journal de Pharm. Mars* 1839.

ACTION OF CHLORIDE OF TIN ON SULPHUROUS ACID. BY M. HERING.

It is well known that sulphurous acid, although one of the most powerful reducing bodies, is nevertheless inferior to chloride of tin in this respect, for a solution of sulphurous acid is completely reduced by this chloride; it is on this energetic reducing property that is founded the known method of trying whether the muriatic acid of commerce contains sulphurous acid. In fact, if to a solution of sulphurous acid, mixed with a large quantity of hydrochloric acid, a small quantity of solution of chloride of tin be added, the liquor though at first perfectly clear becomes turbid after some time, and then assumes, at first a sulphur yellow colour, and afterwards a deeper one. This action of chloride of tin is almost instantaneous when the mixed liquors are slightly heated, and then the odour of sulphurous also ceases.

The yellow precipitate has usually been considered as a mixture of oxide of tin and sulphur; according to this opinion, an equivalent of chlorine must be displaced by two equivalents of oxygen; and there would occur at the same time the separation of an equivalent of sulphur and the formation of chloride of tin.

It was nevertheless natural to presume that the precipitate in question might also be a peculiar sulphuret of tin. In order to determine this, a portion of a solution of sulphurous acid was mixed with hydrochloric acid, and reduced in the manner stated by chloride of tin, with the aid of heat. The precipitate obtained, after being well washed and dried at a gentle heat, was subjected to the following experiments.

Put into caustic potash, it completely dissolved after some time, and the solution assumed a yellow colour; the acetic and hydrochloric acids produced in it a yellow precipitate without any evolution of sulphuretted hydrogen; after remaining some time in ammonia it was completely dissolved, with the exception of a slight trace, and the solution was of a deep yellow colour; the solution was decomposed by the addition of acetic or hydrochloric acid: when agitated and left for a long time in contact with sulphuret of carbon, no change occurred either in colour or bulk; the solution, which was separated by filtration, left but slight traces of sulphur by spontaneous evaporation; the precipitate dissolved readily and without residue in sulphuret of ammonia; acids precipitated yellow sulphuret of tin and sulphur, accompanied with the disengagement of sulphuretted hydrogen.

Hydrochloric acid dissolved it gradually with the assistance of heat, and gave out a great quantity of sulphuretted hydrogen, but slight traces of sulphur were separated; the solution with potash and ammonia yielded white precipitates, soluble in an excess of these alkalis.

According to these researches, the substance produced by the reaction of chloride of tin on sulphurous acid is a sulphuret of tin. M. Hering, conceiving that he should arrive at a more perfect knowledge of its composition by combustion, heated 228 parts in the air a small

porcelain capsule; the mean of two experiments left 182 parts of white powder; and the atomic weight of sulphuret of tin being 1137·63 and that of oxide of tin 935·29, the sulphuret in question left a quantity of oxide nearly corresponding to that of the sulphuret.

The result of the reaction of chloride of tin on sulphurous acid, consists then in the precipitation of sulphuret of tin on one hand, and the production of chloride of tin on the other: 2 at. of sulphurous acid and 3 at. of chloride of tin are decomposed into 2 at. of oxide of tin and 6 at. of free chlorine: these last combine with 3 at. of chloride of tin to form the same number of atoms of chloride, and the two atoms of oxide of tin form chloride of tin with the free hydrochloric acid.—*Journal de Pharm. Mars* 1839.

SPONTANEOUS DECOLORATION OF TINCTURE OF LITMUS. BY M. VOGEL.

It frequently happens that the tincture of litmus prepared with boiling water loses its blue colour entirely after a time, and becomes of a bright brown, or wine yellow colour. This alteration of colour has already been observed by M. Desfosses (Journal de Pharmacie, xiv. p. 487) and by M. Chevreul, and undoubtedly also by other chemists.

The decoloration occurs especially when the tincture has been left for several months undisturbed, and well stopped in bottles which are not completely full: with alcohol the tincture decolorates more slowly than without it, and the decoloration is especially favoured where a quantity of several pounds is kept in a bottle.

The tincture thus becomes yellow, is not spoiled by the change, nor is it unfit for use, for its original colour may be made to reappear in several modes; first by exposing it to the air, or by agitating it in a bottle with air. Its colour reappears also when heated to 122° Fahr. in a receiver over mercury, provided some air be present in the receiver.

Although it appears probable that the tincture which has been spontaneously decolorated, becomes again blue by the oxidation of the air, (for it forms at first a blue ring on the surface of the liquid,) it requires however so small a quantity of oxygen, that I could scarcely perceive any diminution in the volume of the air, whilst it regained its colour.

As the litmus of commerce contains a trace of animal matter, I presumed at first that the decoloration was excited by the decomposition of this animal substance, and that carbonate of ammonia was formed; but experiment did not confirm this suspicion, for on heating the tincture, which had become spontaneously yellow, in a mattrass furnished with a bent tube, neither ammonia nor carbonic acid was evolved, although the liquid became again blue by the increase of temperature.

As the litmus of commerce almost always contains sulphate of potash also, it appeared to me possible, and even probable, that if this salt were decomposed, the decoloration might be the result of it. I ascertained the presence of sulphate of potash in the litmus on which my experiments were made, in the following manner: I

added chloride of barium to the tincture made with boiling water; it formed an abundant blue precipitate, and the liquor was entirely decolorated at the expiration of 24 hours.

The washed precipitate was of a deep blue colour, and it had in part the properties of a compound of the blue colour of the litmus with barytes. To examine whether the dried precipitate contained any sulphate of barytes, it was heated to redness in a platina crucible, and moistened with hydrochloric acid, which disengaged sulphuretted hydrogen gas. Besides this I evaporated the tincture of litmus to dryness, and then heated the residue to redness; the ashes, besides carbonate of potash and chloride of potassium, contained some sulphate of potash.

The gradual decomposition of sulphate of potash by the organic matter, and especially the sulphuretted hydrogen which results from it, appears then to be the principal cause of the decoloration of the tincture of litmus; nevertheless, as in pursuing these experiments I did not discover the presence of sulphuretted hydrogen in the decolorated tincture, by employing paper moistened with acetate of lead, I became uncertain whether the decoloration was really effected by sulphuretted hydrogen. As however a few drops of an aqueous solution of sulphuretted hydrogen, added to a large quantity of the blue tincture, well stopped in a bottle, were sufficient to decolorate it in a few days, and as I could not discover in the tincture thus decolorated, any sulphuretted hydrogen, it having been decomposed, I had no longer any doubt that the blue colour of the tincture was destroyed, under all circumstances, by the sulphuretted hydrogen which is insensibly formed; thus taking away a part of the oxygen which it afterwards absorbs from the air, and its blue colour returns. After what has been stated, it was impossible to prove the presence of sulphuretted hydrogen in the decolorated tincture, because it is decomposed immediately after its formation.

The decoloration of the tincture of litmus by means of a few drops of solution of sulphuretted hydrogen, and the recovery of its blue colour by the contact of air, may be repeated a great number of times, without the tincture seeming to undergo any sensible change. When a small quantity of sulphate of lime or sulphate of soda is dissolved in the tincture of litmus, the decoloration begins sooner than with the sulphate of potash which exists in the tincture. Brasilin dissolved in water and stopped in a bottle with sulphuretted hydrogen is also decolorated, but hematin requires a long time for the production of the effect; and the infusion of the leaves of the *Delphinium Ajacis* does not undergo any sensible change by sulphuretted hydrogen, even after the lapse of several weeks.

Journal de Pharm. Mars 1839.

ROYAL ACADEMY OF SCIENCES OF PARIS.

On Monday, June 24, the Rev. Dr. Buckland, Canon of Christ Church, Professor of Mineralogy and Geology in the University of Oxford, and President of the Geological Society of London, was

elected one of the eight corresponding members of the Institute of France, in the section of mineralogy.

The Academy of France has also awarded the sum of 3000 francs to Professor Doyere of the College of Henry IV. for his translation of Dr. Buckland's Bridgewater Treatise, and has placed this work on the list of prize books to be distributed in the Colleges of France.

M. Villemain, in his report to the French Academy (Journal des Debats, 31 May 1839), states that this prize is awarded to M. Doyere for having published "la meilleure Traduction d'un ouvrage de Morale," that has appeared during the last year. A similar reward has also been given to M. Thurot for his translation of Epictetus into French.

DR. BARRY'S RESEARCHES IN EMBRYOLOGY.

We are requested by Dr. Martin Barry to state, that at the time of presenting his "second series" of "Researches in Embryology" to the Royal Society (of which an abstract was given in our supplementary number for July of the present year), he was not aware that the fact regarding spermatozoa reaching the surface of the ovary had been previously discovered by Dr. Bischoff, of Heidelberg.

METEOROLOGICAL OBSERVATIONS FOR JUNE, 1839.

Chiswick.—June 1. Overcast: fine. 2. Dry haze. 3. Foggy: rain. 4. Foggy: cloudy. 5. Very fine: heavy rain at night. 6. Very fine. 7. Rain. 8—13. Very fine. 14—15. Hazy. 16. Very fine. 17. Very fine: thunder at night. 18. Slight haze: cloudy, with thunder. 19. Very fine: lightning at night. 20. Very hot. 21. Cloudy and fine. 22. Showery. 23. Stormy with rain. 24. Cloudy. 25. Very fine: rain. 26. Sultry: thunder. 27. Fine. 28. Thunder showers. 29, 30. Cloudy and cold.

Boston.—June 1, 2. Cloudy. 3. Rain: rain early A.M. 4, 5. Cloudy. 6. Fine: 3 o'clock P.M. therm. 72°. 7. Cloudy: rain P.M. 8—10. Fine. 11. Cloudy. 12. Fine. 13. Cloudy: rain P.M. 14. Rain: heavy rain with lightning early A.M.: rain again A.M. and P.M. 15—17. Cloudy. 18—20. Fine. 21. Cloudy: rain early A.M. 22. Fine: rain A.M. and P.M. 23. Cloudy: rain P.M. 24. Fine: rain early A.M. 25. Fine. 26. Rain. 27. Cloudy; rain P.M., with clap of thunder. 28. Rain. 29. Cloudy: rain P.M. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—June 1, 2. Dry and withering. 3. Getting cloudy. 4. Very warm: air electrical. 5. Cleared up: soft and warm. 6. Pleasant day: moderate breeze. 7. Gentle rain all day. 8. Fair and droughty. 9. Fine day, but parching. 10. A welcome rain P.M. 11. Fine growing day: ground refreshed. 12. Genial rain: vegetation strong. 13. Rather cool A.M.: rain P.M. 14. Fair: growing day. 15, 16. Very warm and genial. 17. The same: thunder and rain P.M. 18. Very warm, but getting cloudy. 19. The same: rain P.M. 20. The same: getting cloudy. 21. Rain nearly all day. 22. Rain, soft and genial. 23. Showery all day. 24. Dull day, but kept dry. 25. Fine summer day. 26. Cloudy morning: cleared up. 27. Thunder, with heavy showers. 28, 29. Fair: temperature cool. 30. Beautiful summer day.

Sun shone out 26 days. Rain fell 10 days. Thunder 2 days.

Wind southerly 12 days. Westerly 6 days. Easterly 6 days. Northerly 6 days.

Calm weather 9 days. Moderate 12 days. Brisk 8 days. Boisterous 1 day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. June.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr 9 a.m	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8¾ a.m.	Thermometer. Dumfries-shire. 9a.m.	Thermometer. Dumfries-shire. 9pm.	Wind. London: Roy.Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy.Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew point. Lond.: Roy. Soc. 9 a.m.
1.	29·964	29·969	29·927	29·58	30·07	30·03	55·2	55·7	48·2	71	45	52	52	44	N.	E.	NE.	NE.	...	...	...	...	52
2.	29·906	29·906	29·810	29·45	29·95	29·87	53·7	68·0	45·6	64	46	51	53	46½	NE.	SE.	calm	E.	...	·33	...	...	48
3.	29·694	29·708	29·647	29·23	29·75	29·70	50·8	51·3	47·0	66	49	48	53½	48	ENE.	S.	E.	E.	·227	·20	·22	...	48
☾ 4.	29·666	29·736	29·654	29·15	29·69	29·67	54·6	54·7	50·3	66	46	53·5	59	56	E.	NE.	calm	W.	·069	...	·10	...	51
5.	29·836	29·903	29·822	29·25	29·76	29·83	59·3	61·7	53·3	72	46	57·5	60½	57	N.	S.	calm	W.	...	·64	...	0·40	52
6.	29·970	29·950	29·937	29·38	29·83	29·80	63·9	57·9	54·2	71	53	65	63	55½	S.	SW.	calm	W.	·227	·02	...	...	55
7.	29·848	29·843	29·393	29·19	29·63	29·63	60·3	69·3	56·3	64	49	61	57½	57	E.	S.	calm	W.	·033	·17	...	...	56
8.	29·898	29·901	29·850	29·27	29·69	29·72	63·2	63·7	55·9	72	52	65	59	53½	SE. var.	S.	SW.	S.	·091	...	·09	...	56
9.	30·058	30·215	30·016	29·40	29·70	29·95	63·8	85·0	56·0	72	48	65	58	52	S.	S.	W.	S.	...	...	...	...	57
10.	30·318	30·291	30·236	29·68	30·06	29·93	63·9	71·2	55·5	71	56	64	59	53	SW.	SW.	W.	S.	...	...	...	...	56
● 11.	30·262	30·270	30·223	29·55	30·05	30·17	64·7	70·0	59·4	70	49	66	59	56	W.	W.	W.	W.	...	...	...	...	59
12.	30·244	30·225	30·002	29·55	30·04	30·07	67·3	80·7	57·8	81	56	68	53	49	S.	S.	SW.	NE.	...	...	...	1·06	59
13.	29·944	29·911	29·870	29·40	30·05	30·05	72·0	78·3	62·2	81	53	58	56½	48	NE.	E.	E.	NE.	...	...	...	...	65
14.	29·866	29·889	29·846	29·42	30·03	30·02	58·2	77·6	55·3	64	55	51	53	50	NE.	E.	E.	E.	...	...	·87	...	57
15.	29·946	30·171	29·929	29·47	30·03	30·13	58·4	64·4	55·7	63	47	52·5	60½	59	NNW.	NE.	calm	S.	...	...	1·28	...	58
16.	30·268	30·279	29·936	29·77	30·23	30·25	61·3	65·0	49·0	73	50	56	65	63	NE.	E.	calm	S.	...	...	...	...	53
17.	30·200	30·195	30·093	29·72	30·24	30·11	64·2	70·3	52·5	75	54	57	71	65	ENE.	NE.	calm	SSE.	...	...	...	...	58
☽ 18.	30·054	30·025	29·940	29·46	29·99	29·92	70·6	83·6	61·6	84	58	66	69½	63	E.	SE.	calm	S.	...	·16	...	...	65
19.	29·998	30·140	29·948	29·33	29·89	30·00	66·6	82·3	62·0	76	45	68	62	62	S.	SW.	W.	S.	·050	...	...	...	62
20.	30·158	30·134	29·857	29·53	30·08	29·95	67·8	85·0	58·5	83	59	70	59	63	S.	SE.	calm	E.	...	·01	...	0·97	62
21.	29·818	29·807	29·764	29·10	29·68	29·60	68·3	76·2	61·0	72	56	65	57	50½	S.	SW.	calm	NE.	·033	...	·02	...	62
22.	29·644	29·642	29·409	29·03	29·46	29·26	63·9	73·6	59·9	66	55	65	60	53½	S.	S.	SW.	SE.	...	·11	...	...	60
23.	29·456	29·702	29·414	28·78	29·23	29·42	62·8	70·0	56·8	65	54	61	54	53	SE. var.	SW.	W.	SW.	·111	·18	·25	...	57
24.	29·842	29·896	29·797	29·16	29·52	29·74	63·4	76·4	55·6	70	54	62	56	58	S.	SW.	W.	W.	·319	...	·01	...	58
25.	29·968	29·943	29·661	29·36	29·82	29·80	64·3	69·3	57·4	74	55	63	61	54	S.	S.	calm	S.	...	·07	...	...	57
○ 26.	29·552	29·566	29·470	29·06	29·67	29·64	63·4	70·4	56·0	72	51	59	50	54	E.	SW.	calm	E.	...	·51	·16	...	58
27.	29·892	29·861	29·743	29·27	29·72	29·72	62·8	68·6	54·2	70	50	62	58	48	W.	SW.	calm	SE.	·083	·20	·41	1·10	57
28.	29·684	29·841	29·663	29·14	29·77	29·89	61·3	81·2	54·7	54	49	55	56	51	S.	W.	calm	EbyN.	·133	·40	·25	...	57
29.	29·956	30·139	29·944	29·46	30·05	30·17	53·7	54·0	50·3	51	41	54	53	48	WNW.	N.	N.	N.	·144	...	·85	...	52
30.	30·158	30·285	30·150	29·68	30·19	30·20	53·3	53·8	45·8	56	40	51	56	54	W.	NW.	N.	N.	...	...	·07	...	46
Mean.	29·936	29 ·978	29·831	29·36	29·86	29·87	61·9	70·2	54·9	68·30	50·70	59·7	58·1	54·1					Sum. 1·520	3·00	4·58	3·53	Mean. 56·4

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XXIX. *On the Method of producing Copies of engraved Copper-plates by Voltaic Action; on the supply of mixed Gases for* Drummond's *Light, by Electrolysis; on the Application of Electro-magnetism as a motive power in Navigation, and on Electro-magnetic Currents. By* Dr. M. H. Jacobi: *in a Letter to Mr.* Faraday.*

IT is some time since, that during my electro-magnetic labours, a fortunate accident conducted me to the discovery that we might by voltaic action make copies in relief of an engraved copper plate, and that a new inverted copy of those in relief might be obtained by the same process, so that the power was obtained of multiplying the copper copies to any extent. By this voltaic process the most delicate and even microscopic lines are reproduced, and the copies are so identical with the original that the most rigorous examination cannot find the least difference. I send you in the accompanying packet two specimens of such plates, which I hope you will accept with kindness. The one which is in relief is the copy of an original engraved with the burin; the second is the copy of that in relief, and consequently identical with the original. The third is the original plate, but covered with reduced copper. I had the intention of making a second copy, but unfortunately the plates adhere so strongly at times that it is impossible to separate them. I cannot tell the cause of this intimate union which occasionally occurs, but it appears to be the case only when the copper at the surface of which the reduction is effected is brittle, and consequently is lamellar and porous. I may dispense with describing more at large the apparatus that I make use of. It is simply a voltaic pair

* Communicated by Dr. Faraday.

à cloison where the engraved plate is used in the place of the ordinary copper plate, being plunged in the solution of sulphate of copper. I have found it necessary that a galvanometer with short wires should always make part of the circuit, so that one may judge of the force of the current and direct the action; the latter being effected by separating the electromotive plates more or less from each other or modifying the length of the conjunctive wire, or finally, diminishing more or less the conducting power of the liquid on the zinc side; but for the success of the operation it is of great importance that the solution of copper should be always perfectly saturated. The action should not be too rapid: from 50 to 60 grains of copper should be reduced on each square inch in 24 hours. The accompanying plates have been formed, one in two days, the other in one day only, and that is the reason why their state of aggregation is not so solid and compact as that of the small piece, No. 4, which has been reduced more slowly.

It is to be understood that we may reduce the sulphate of copper by making the current of a single voltaic pair pass through the solution by copper electrodes; as the anode is oxidized the cathode becomes covered with reduced copper, and the supply of concentrated solution may then be dispensed with. According to theory one might expect that exactly the same quantity of copper oxidized on one side would be reduced on the other, but I have always found a difference more or less great, so that the anode loses more than the cathode gains. The difference appears to be nearly constant, for it does not augment after a certain time, if the experiment be prolonged. A thoroughly concentrated solution of sulphate of copper is not decomposable by electrodes of the same metal, even on employing a battery of three or four pairs of plates. The needle is certainly strongly affected as soon as the circuit is completed, but the deviation visibly diminishes and very soon returns almost to zero. If the solution be diluted with water to which a few drops of sulphuric acid have been added, the current becomes very strong and constant, the decomposition goes on very regularly, and the engraved cathode becomes covered with copper of a fine pink red colour. If we replace the solution of sulphate of copper by pure water acidulated with sulphuric acid, there is a strong decomposition of water even on employing a single voltaic couple. The anode is oxidized, and hydrogen is disengaged at the cathode. At the commencement the reduction of copper does not take place; it begins as soon as the liquid acquires a blue colour, but its state of aggregation is always incoherent. I have continued this experiment for three

days, until the anode was nearly dissolved; the colour of the liquid became continually deeper, but the disengagement of hydrogen, though it diminished in quantity, did not cease. I think we may conclude from this experiment that in secondary voltaic actions there is neither that simultaneity of effect, nor that necessity of entering into combination or of being disengaged from it, which has place in primary electrolytic actions.

During my experiments many anomalies respecting these secondary actions have presented themselves which it would be too embarrassing to describe here: in fact there is here a void which it will be difficult to fill, because molecular forces which as yet we know nothing of appear to play a most important part.

With respect to the technical importance of these voltaic copies, I would observe that we may use the engraved cathode, not only of metals more negative than copper, but also of positive metals and their alloys, (excepting brass,) notwithstanding that these metals, &c. decompose the salts of copper with too much energy when alone. Thus one may make, for example, stereotypes in copper which may be multiplied as much as we please. I shall shortly have the honour to send you a bas-relief in copper, of which the original is formed of a plastic substance, which adapts itself to all the wants and caprices of art. By this process all those delicate touches are preserved which make the principal beauty of such a work, and which are usually sacrificed in the process of casting, a process which is not capable of reproducing them in all their purity. Artists should be very grateful to galvanism for having opened this new road to them.

During the last winter I frequently illuminated my saloon, which is of considerable size, by Drummond's light. The mixed gases were obtained in sufficient quantities, that is to say, at the rate of 3 or 4 cubic feet per hour, by decomposing dilute sulphuric acid (specific gravity 1·33,) between electrodes of platina by a constant battery of a particular construction. I only passed the gas through a glass tube filled with chloride of calcium, and there was neither gasometer nor any other provision for it. As soon as the voltaic current was closed the jet might be lighted, and the flame then burnt tranquilly, and of the same intensity for any length of time. The construction and manipulation of the battery, though extremely perfect, was still a little embarrassing. At present, a battery, with a decomposing apparatus which will produce from 3 to 4 cubic feet of electrolyzed gas per hour, occupies little more space than the page of paper on which I write to you (10

inches by 8 inches) and is about 9 inches in height. Behold certainly a beautiful application of the voltaic battery.

In the application of electro-magnetism to the movement of machines, the most important obstacle always has been the embarrassment and difficult manipulation of the battery. This obstacle exists no longer. During the past autumn and at a season already too advanced, I made, as you may perhaps have learned by the gazettes, the first experiments in navigation on the Neva, with a ten-oared shallop furnished with paddle-wheels, which were put into motion by an electro-magnetic machine. Although we journeyed during entire days, and usually with 10 or 12 persons on board, I was not well satisfied with this first trial, for there were so many faults of construction and want of insulation in the machines and battery which could not be repaired on the spot, that I was terribly annoyed. All these repairs and important changes being accomplished the experiments will shortly be recommenced. The experience of the past year combined with the recent improvements of the battery give as the result, that to produce the force of one horse (steam-engine estimation) it will require a battery of 20 square feet of platina distributed in a convenient manner, but *I hope* that from 8 to 10 square feet will produce the effect. If heaven preserves my health, which is a little affected by continual labours, I hope that within a year of this time, I shall have equipped an electro-magnetic vessel of from 40 to 50 horse power.

In my paper, "On the application, &c.*" I have spoken of the influence which those magneto-electric currents which you had discovered a short time before, would exert on the progress of electro-magnetic machines. They are properly the cause that the expectations which have been entertained regarding these machines have not as yet been fulfilled. But if one examines them more nearly these currents are not so disadvantageous as have been supposed. Experiments which I have made by interposing a galvanometer or a voltameter have taught me that during the action of the machine the electrolytic action of the battery is much less, and sometimes not more than half that which takes place when the machine is stopped, the current still passing by the helices which surround the bars of iron. Thus if on the one part the magneto-electric currents diminish the force of the machine, on the other the electrolytic dissolution of the zinc, which makes the greatest part of the current expense, is at the same time considerably diminished. I have not as yet succeeded in com-

* See Taylor's SCIENTIFIC MEMOIRS, vol. i. p. 503, and vol. ii. (Part V.) p. 1.—EDIT.

pletely developing the mutual relations of the current before and during the working of the machine.

I take the liberty of sending you some memoirs from the *Bulletin scientifique* of the Academy. The result of the joint memoir of myself and M. Lenz is *that the attraction of electro-magnets is as the square of the force of the current, or as the square of the electrolytic action of the battery.* It appears that this important law holds good for machines in motion; at least the experiments I have made on that point do not depart from it more than may be admitted as the error of observation or the result of accidental circumstances.

I am, &c.

St. Petersburg, June 21, 1839. M. H. Jacobi.

XXX. *On the general Solution of Algebraical Equations. By* J. W. Lubbock, *Esq., F.R.S.**

Let $x^n + \ldots\ldots Cx^2 + Dx + E = 0 \quad \ldots\ldots\ldots \quad (1.)$ be any equation, and let

$$x = p + qy + ry^2 + sy^3 \ \ldots\ldots + \&c. \ldots\ldots \quad (2.)$$

y being raised to the $n-1^{\text{th}}$ power in the last term of equation (2.). Let $fy = 0$ (3.), and let α, β, γ be the roots of equation (3.). Moreover, let

$$\Sigma_1 = \alpha + \beta + \gamma \ldots \&c. \qquad \Sigma_2 = \alpha^2 + \beta^2 + \gamma^2 + \&c.$$
$$f_1 = a + b + c \ldots \&c. \qquad f_2 = a^2 + b^3 + c^2 + \&c.$$

$a, b, c,$ &c. being the roots of equation (1.).

The equation which arises from the elimination of y between (2.) and (3.) will be, as is well known, that formed by the product (*Francœur*, vol. ii. p. 126.)

$$\{x - p - q\,\alpha - r\,\alpha^2 \ \ldots\ldots \ + \&c.\}$$
with
$$\{x - p - q\,\beta - r\,\beta^2 \ \ldots\ldots \ + \&c.\}$$

and all similar quantities; and as this equation must be identical with equation (1.),

$$\{x-p-q\,\alpha-r\,\alpha^2 \ldots + \&c.\}$$
$$\{x-p-q\,\beta-r\beta^2 \ldots + \&c.\} = x^n \ldots + Cx^2 + Dx + E = 0.$$

Dividing both sides of the last equation by x^n, taking the logarithms and equating the coefficients of the same powers of x, we obtain at once the equations of condition which exist between the quantities $p, q, r,$ &c., $\Sigma_0, \Sigma_1, \Sigma_2,$ &c., and the coefficients C, D, E, &c. of the proposed equation.

Now let equation 3. be $y^n - 1 = 0$, then

$$\Sigma_0 = n, \ \Sigma_1 = 0, \ \Sigma_2 = 0, \ \&c.$$

* Communicated by the Author.

The rule now is, raise

$$p + q z + r z^2 + \&c.$$

to the mth power, make all powers of z equal to zero in the result (in consequence of the nature of the roots of unity) except such as are multiples of n, in these write n for z^0, z^n, z^{2n}, &c.; the result is m times the coefficient of x^{-m} in the logarithm of the left hand side divided by x^n of the last equation with a contrary sign. The equations of condition thus found between p, q, r, &c. and C, D, E admit generally of reductions, and in this manner uniform solutions of the quadratic, cubic, and biquadratic equations may readily be obtained. The same method may also be extended to the equation of five dimensions, so far at least as to exhibit the analogous equations between p, q, r, s, t, and C, D, E. This method was first given by Bezout in the *Mémoires de l'Académie* for 1765.

Passing over the *Solution of the quadratic equation*, I proceed to the

Solution of the cubic equation.

$$x^3 + \mathrm{D}\,x + \mathrm{E} = 0 \quad \ldots\ldots\ldots\ldots\ldots \quad (1.)$$

I assume $x = p + q\,y + r\,y^2$ (2.) $y^3 = 1$ (3.)

$$\alpha = 1 \quad \beta = -\tfrac{1}{2} + \frac{\sqrt{-3}}{2} \qquad \gamma = -\tfrac{1}{2} - \frac{\sqrt{-3}}{2}.$$

The first equation of condition gives $p = 0$, which is the case whenever the second term of the equation 1. is wanting. I therefore take $x = q\,y + r\,y^2$,

$$\left\{1 - \frac{q\,\alpha + r\,\alpha^2}{x}\right\}\left\{1 - \frac{q\,\beta + r\,\beta^2}{x}\right\}\left\{1 - \frac{q\,\gamma + r\,\gamma^2}{x}\right\}$$

$$= 1 + \frac{\mathrm{D}}{x^2} + \frac{\mathrm{E}}{x^3}.$$

The equations of condition in this case are $-q\,r = \dfrac{\mathrm{D}}{3}$

$$-q^3 - r^3 = \mathrm{E}$$

$$r^6 + \mathrm{E}\,r^3 - \frac{\mathrm{D}^3}{27} = 0 \qquad r^3 = -\frac{\mathrm{E}}{2} + \sqrt{\frac{\mathrm{E}^2}{4} + \frac{\mathrm{D}^3}{27}}$$

$$x = -\frac{\mathrm{D}}{3}\left\{-\frac{\mathrm{E}}{2} + \sqrt{\frac{\mathrm{E}^2}{4} + \frac{\mathrm{D}^3}{27}}\right\}^{-\frac{1}{3}} y$$

$$+ \left\{-\frac{\mathrm{E}}{2} + \sqrt{\frac{\mathrm{E}^2}{4} + \frac{\mathrm{D}^3}{27}}\right\}^{\frac{1}{3}} y^2$$

which agrees with the well-known solution.

Solution of the Biquadratic Equation.

$$x^4 + \mathrm{D}\,x + \mathrm{E} = 0 \qquad (1.)$$

I assume $x = qy + ry^2 + sy^3$ (2.) $\quad y^4 - 1 = 0$ (3.)

The equations of condition in this case are

$$r^2 + 2qs = 0 \qquad -4q^2r - 4rs^2 = \text{D}.$$

$$q^4 + 6q^2s^2 + 12qr^2 + r^4 + s^4 = -\text{E}.$$

These are the equations given by Bézout, *Cours de Mathématiques*, vol. iii. p. 220.

$$q^2 + s^2 = -\frac{\text{D}}{4r}, \text{ and after reductions}$$

$$(q^2 + s^2)^2 - 16q^2s^2 = \text{E} \qquad \frac{\text{D}^2}{16r^2} - 4r^4 = \text{E}.$$

$r^6 + \frac{\text{E}}{4}r^2 - \frac{\text{D}^2}{64} = 0$, from which r^2 is found by the solution of a cubic. q and s can then be easily obtained.

Proceeding to treat in this manner the *equation of five dimensions*,

$$x^5 + \text{C}x^2 + \text{D}x + \text{E} = 0 \qquad (1.)$$

I assume

$$x = qy + ry^2 + sy^3 + ty^4 \quad (2.) \quad y^5 - 1 = 0 \quad (3.)$$

The equations of condition in this case are

$$qt + rs = 0$$

$$q^2r + qr^2 + rt^2 + s^2t = -\frac{\text{C}}{5}$$

$$4q^3r + 6q^2t^2 + 24qrst + 4qs^3 + 4r^3t + 6r^2s^2 = -\frac{4\text{D}}{5}$$

$$q^5 + 20q^3st + 30q^2r^2t + 30q^2rs^2 + 20qr^3s + r^5$$
$$+ 20qrt^3 + 30qs^2t^2 + 30rs^2t + 20rs^3t + s^5 + t^5 = -\text{E}.$$

The two last equations admit of reduction, and finally the solution of the equation of five dimensions may be said to depend upon the determination of the quantities q, r, s, t from the following equations:

$$qt + rs = 0 \qquad q^2s + qr^2 + rt^2 + s^2t = -\frac{\text{C}}{5}$$

$$q^3r + 3qrst + qs^3 + r^3t = -\frac{\text{D}}{5}$$

$$q^5 - 20qr^3s - 20rs^3t + r^5 + s^5 + t^5 = -\text{E} - \frac{qt\text{C}}{5}.$$

The same method would serve to transform equation (1.) into any other equation $fy = 0$ of the same number of dimensions, but the simplicity of the equations of condition which are finally obtained, depends upon the nature of the quantities Σ_1, Σ_2, Σ_3, &c. and scarcely at all upon the nature

of the quantities f_1, f_2, f_3, &c., or the number of terms which are wanting in equation 1.

I apprehend that the equations of condition which result when we attempt to transform in this manner equation (1.) into the equation $y^5 - 1 = 0$, are far simpler than those which obtain when we endeavour to transform equation (1.) into the equation, also solvible,

$$y^5 + \mathrm{B}\, y^3 + \frac{1}{5}\, \mathrm{B}^2 y + \mathrm{E} = 0$$

or indeed into any other.

Instead of assuming

$$x = p + q\, y + r\, y^2 + \&c.$$

we might assume

$$y = p' + q'\, x + r'\, x^2 + \&c. \qquad f y = 0 \quad (3.)$$

x being raised to the $n-1^{\text{th}}$ power in the last term of the expression for y. In order to eliminate x between this equation and equation, (1.) we must form the product

$$\{ y - p' - q' a - r' a^2 - \ldots \&c. \}$$
$$\{ y - p' - q' b - r' b^2 \ldots\ldots \&c. \} = f y = 0.$$

And as this equation must be identical with equation (3.), dividing by y^n and taking the logarithms as before, we may find at once the equations of condition which obtain between the quantities p', q', r', &c. f_1, f_2, f_3, &c. and the coefficients of the equation (3.). Suppose for example equation (3.) is $y^5 - 1 = 0$, we get

$$p' f_0 + q' f_1 + r' f_2 + s' f_3 + t' f_4 = 0 \ \&c. \ \&c.$$

and generally the coefficient of y^{-m} on the left hand side of the equation between the logarithms will be found by raising

$$\{ p' + q' z + r' z^2 + s' z^3 + \&c. \}^m$$

and writing f_n instead of z^n in the result, which will be m times the coefficient of y^{-m} in the logarithm of the left hand side divided by y^n of the last equation with a contrary sign. The simplicity of the equations which result between p', q', r', &c. will now depend greatly upon the nature of the roots of the proposed equation and upon the number of terms which are wanting in it. But they will, I believe, generally prove less simple than those which obtain between the quantities p, q, r, &c. and the roots of the proposed equation, and of which I have given some examples.

I have offered these remarks upon the general solution of equations because it is of importance to cultivate general methods. The solutions of the quadratic, cubic, and biquadratic equations according to the method here given are as simple as those, devoid of any connecting principle, which are usually contained in elementary works on Algebra.

XXXI. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel RICHARD WRIGHT, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 104.]

Honda, on the River Magdalena. Lat. 5° 16′ *N.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1822.				
Nov. 1.	76°	6 a.m.	About	
,,	83	9	1200 ft.	Showers at night.
,,	90	12	Situation	
,,	93	2 p.m.	remark-	
2.	77	7 a.m.	ablyclose.	
,,	86	1 p.m.		Fine.
,,	92	3 p.m.		
3.	76	6 a.m.		Id.
,,	86	2 p.m.		Id.
3 days. 88°·33 max. 76 ·33 min. Medium 82°·33.				

Valley of the River Magdalena.

8.	76	6 a.m.	Elevation	River.
,,	86	2 p.m.	of the	
12.	77	7 a.m.	river be-	Village of San Pablo.
,,	88	2 p.m.	tween	
20.	77	7 a.m.	Honda &	Town of Chiriguana.
,,	89	3 p.m.	Mompox	
			639 feet.	
3 days 87°·66 max. 76 ·66 min. 82° 66′ med. Weather fine.				

Rio Hacha, Atlantic Coast. Lat. 11° 28′ *N.*

Dec. 9.	80·5	6 a.m.	0	Fair.
10.				
11.	86	3 p.m.		Id.
12.	76	7 a.m.		
,,	85	3 p.m.		Id.
13.	79	7 a.m.		
14.	86	3 p.m.		
15.	75	7 a.m.		
,,	86	3 p.m.		
22.				
23.	75	7 a.m.		Showers.
24.	80	3 p.m.		
25.				

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1822. Dec. 26, to 31.	80° 86	7 a.m. 3 p.m.		Fine.
22 days 85°·14 max. 78°·27 min. } 81°·70 med.				
1823. Jan. 1 to 8.	77 85	7 a.m. 3 p.m.		Fine, and strong winds.
9. ,,	77 81	7 a.m. 3 p.m.		Id. id.
10 to 13.	77 83	7 a.m. 3 p.m.		Id. id.
14 to 18.	77 82	7 a.m. 3 p.m.		Id. id.
19. ,,	77 87	7 a.m. 3 p.m.		Id. id.
20 to 26.	77 85	7 a.m. 3 p.m.		
27.	78	7 a.m.		
28.	87	3 p.m.		Id. id.
29. ,,	80 89	7 a.m. 3 p.m.		
30. ,,	80 85	7 a.m. 3 p.m.		Id. id.
30 days. 84°·88 max. 77°·77 min. } 81°·32 med.				
February. 21 days.	84°·41 max. 79·25 min. } 81° 83′ med.			Weather bright.
March. 31 days.	85°· max. 83°·5 min. } 84° 25′ med.			During the latter part of the month strong winds, and sky clouded in mornings.
April. 30 days.	83°· max. 80°· min. } 81°·5 med.			High winds and sometimes showers.
May. 31 days.	86°· max. 82°·6 min. } 84°·3 med.			Weather calm and sultry with showers.
June.	86°·12 max. 82°·64 min. } 84° 38 med.			Weather bright.
Mean of seven months 82°·65.				

City of Maracaybo. Lat. 10° 43′ *N.*
Situated on the outlet of the Lake of that name, 34 *miles from the coast.*

Date.	Time.	Thermometer.	Elevation.	Remarks.
1823.				
Sept. 1	82°	7 a.m.	Elevation	
to 4.	85	3 p.m.	inconsi-	Bright.
5.	82	7 a.m.	derable.	
,,	87	3 p.m.	There is	Id.
6.	82	7 a.m.	no sensi-	Id.
7.	90	3 p.m.	ble cur-	Id.
8. 9.	82	7 a.m.	rent from	Id.
and 10.	85	3 p.m.	the lake	Id.
11.	82	7 a.m.	to the	Id.
,,	87	3 p.m.	ocean.	Id.
12.	84	7 a.m.		Id.
,,	94	3 p.m.		Id.
13.	85	7 a.m.		
,,	99	3 p.m.		
14.	82	7 a.m.		
,,	90	3 p.m.		
18.	83	7 a.m.		
,,	92	3 p.m.		
19.	83	7 a.m.		Cloudy.
,,	87	3 p.m.		
20.	83	7 a.m.		Bright.
,,	91	3 p.m.		Id.
21.	83	7 a.m.		Id.
22.	90	3 p.m.		Id.
23.	83	7 a.m.		
,,	91	3 a.m.		Id. Storm at night.
24.	83	7 a.m.		
,,	92	3 p.m.		Bright.
25.	78	7 a.m.		
,,	91	3 p.m.		Id.
26.	81	7 a.m.		
,,	90	3 p.m.		
27.	82	7 a.m.		
,,	91	3 p.m.		
28.	82	7 a.m.		Id.
29.	92	3 p.m.		Id.
30.	82	7 a.m.		Id.
,,	94	3 p.m.		
		90°·42 max. 82 ·42 min. } 86°·42 med.		
Oct. 1.	81	7 a.m.		Id.
,,	87	3 p.m.		
2.	81	7 a.m.		Id.
,,	92	3 p.m.		
3. 4.	82	7 a.m.		
and 5.	90	3 p.m.		Id.

Table *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1823. Oct. 6.	82°	7 a.m.		Bright.
7.	89	3 p.m.		Id.
8.	80	7 a.m.		Id.
,,	89	3 p.m.		
9.	81	7 a.m.		Id.
10.	90	3 p.m.		Id.
11	81	7 a.m.		Id.
to 14.	92	3 p.m.		
15.	82	7 a.m.		Id.
,,	91	3 p.m.		
16.	82	7 a.m.		Id.
,,	94	3 p.m.		
17.	82	7 a.m.		Id.
18.	91	3 p.m.		Id.
19.	80	7 a.m.		Id.
20.	88	3 p.m.		Id.
25.	81	7 a.m.		Id.
,,	91	3 p.m.		
26.	82	7 a.m.		Id.
,,	92	3 p.m.		Id.
27.	81	7 a.m.		
,,	90	3 p.m.		
28.	78	7 a.m.		Cloudy.
,,	88	3 p.m.		
29.	79	7 a.m.		Storm at night.
,,	89	3 p.m.		
30.	80	7 a.m.		Rain.
,,	89	3 p.m.		
31.	76	7 a.m.		Fair.
,,	87	3 p.m.		
		89°·94 max. / 80 ·05 min.	84°·99 med.	
Nov. 1.	78	7 a.m.		Bright.
,,	88	3 p.m.		
2	82	7 a.m.		
to 11.	92	3 p.m.		Id.
12	80	7 a.m.		
to 15.	90	3 p.m.		Id.
16.	70	7 a.m.		
,,	80	3 p.m.		Storm.
17.	75	7 a.m.		
,,	85	3 p.m.		Fair.
18. 19	79	7 a.m.		
to 20.	87	3 p.m.		Id.
21.	80	7 a.m.		
22, 23	80	7 a.m.		Id.
and 24.	87	3 p.m.		Id.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1823. Nov. 25.	80°	7 a.m.		
"	89	3 p.m.		
26.	79	7 a.m.		Fair.
27, 28	80	7 a.m.		
and 29.	87	3 p.m.		Id.
30.	80	7 a.m.		
"	89	3 p.m.		
	87°·25 max. 78 ·08 min. } 83°·91 med.			
Dec. 1.	78 }	7 a.m.		Cloudy.
"	86 }	3 p.m.		
2.	78	7 a.m.		Clear, with breezes morning and evening.
to 16.	89	3 p.m.		
17, 18,	} 77	7 a.m.		
and 19.	} 87	3 p.m.		Id.
20	} 75	7 a.m.		Fresh winds, day partially clouded.
to 31.	} 85	3 p.m.		At day-break thermometer sometimes descended to 73°.
	86°·75 max. 77 ·00 min. } 81°·87 med.			
1824. Jan. 1	} 75	7 a.m.		Windy and clear.
to 12.	} 87	2 p.m.		
13.	} 79	7 a.m.		Winds cease; close and sultry.
to 24.	} 88	2 p.m.		
25.	79	7 a.m.		
"	89	2 p.m.		
26.	79	7 a.m.		Id. id.
"	88	2 p.m.		
27.	79	7 a.m.		
to 31.	90	2 p.m.		Id. id.
	85°·4 max. 77 ·0 min. } 81° 2′ med.			
February.	89°·15 max. 77 ·57 min. } 83°·36 med.			Weather bright; north winds at the beginning of the mon.
March.	86°·33 max. 79 ·33 min. } 82°·83 med.			From 3rd to 13th cloudy with showers and wind. Therm. 85°·78.
April.	89°·71 max. 83 ·00 min. } 86°·35 med.			Bright.
May.	88°·37 max. 83 ·00 min. } 85°·93 med.			Bright.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1824. June 11.	85°	8 a.m.		Bright.
12.	86	12		
13.	84·5	7 a.m.		Id.
	88	2 p.m.		
14.	85	7 a.m.		Rain.
,,	89·5	2 p.m.		
15.	89	2 p.m.		
16.	85	6½ a.m.		Bright.
,,	91	3 p.m.		
17.	87	7½ a.m.		Id.
,,	89	2 p.m.		
18.	85	7½ a.m.		
,,	92·5	2 p.m.		Id.
19.	85	7½ a.m.		
,,	91	2 p.m.		
20.	81	5½ a.m.		Id.
21.	92	2 p.m.		
22.	82	6 a.m.		
,,	91	2 p.m.		
23.	82	6 a.m.		Cloudy.
24.	88	4 p.m.		
25.	78	6 a.m.		Fair.
,,	91	2 p.m.		
26.	82	6 a.m.		Id.
,,	91	2 p.m.		
27.	81	6 a.m.		
,,	91	2 p.m.		Id.
28.	83	6 a.m.		
,,	91	2 p.m.		Id.
29.	86	6 a.m.		
30.	91	2 p.m.		Id.
		90°·2 max. / 83 ·5 min. } 86·6 med.		
July 1.	86	6 a.m.		Bright.
2.	90	2 p.m.		
3.	83	6 a.m.		Id.
,,	89	2 p.m.		
4.	83	6 a.m.		Id.
to 7.	88	2 p.m.		
8.	83	6 a.m.		
,,	89	2 p.m.		
9.	81·5	4 p.m.		Rain.
to	83	6 a.m.		Fair.
13.	88	2 p.m.		
14.	85	6 a.m.		
	90	2 p.m.		Id.
15.	83	6 a.m.		
16.	88	2 p.m.		Id.

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1824.	°			
July 17.	83	6 a.m.		Fair.
,,	89	2 p.m.		
18.	85	6 a.m.		
to 23.	93	2 p.m.		Id.
24.	85	6 a.m.		
25.	89	2 p.m.		
26.	85	6 a.m.		
to 31.	90	2 p.m.		Id.
	89°·33 max. 84 ·00 min. } 86°·66 med.			
August 10 days.	88°·82 max. 85 ·00 min. } 86°·91 med.			Bright and close.
	Mean from September to December 84°·29, —— from January to August ...... 84 ·98 } Med. 84°·63.			

City of Cartagena. Lat. 10° 24′ *N.*

Aug. 20.	83	6 a.m.		Fair.
21, 22.	86	2 p.m.		
23.	84	6 a.m.		
,,	88	2 p.m.		Id.
24.	84	6 a.m.		
,,	87	2 p.m.		
25.	81	6 a.m.		Id.
,,	84	2 p.m.		
26.	82	6 a.m.		
,,	86	2 p.m.		Id.
27, 28.	} 82	6 a.m.		Showery.
29.	} 86	2 p.m.		
30.	82	6 a.m.		Cloudy.
,,	85	2 p.m.		
31.	82	6 a.m.		Fair.
	87·5	2 p.m.		
	86°·7 max. 82 ·57 min. } 84°·32 med.			

Coast of the Pacific.

I have preferred, in the following Tables, the order of *Place* to that of *Time*, for the purpose of approximating observations made at the same place at different times.

City of Panamà. *Lat.* 9° 00′ 30″ *N.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1824. Sept. 18.	75°	6 a.m.		Showers.
„	78	2 p.m.		
19.	75	6 a.m.		Id.
20.	85	2 p.m.		
21.	78	6 a.m.		
„	85	2 p.m.		
22.	80	6 a.m.		Rain.
„	85	2 p.m.		
23.	79	6 a.m.		Fair.
„	85	2 p.m.		
24.	81	6 a.m.		Id.
25.	86	2 p.m.		
26.	81	6 a.m.		Id.
27.	85	2 p.m.		
28.	80	6 a.m.		
29.	85	2 p.m.		Showery.
31.	81	6 a.m.		
„	86	2 p.m.		Fair.
Oct. 1.	82	6 a.m.		
„	80	2 p.m.		Rain.
2.	80	6 a.m.		Id.
„	82	2 p.m.		
3.	80	6 a.m.		Fair.
„	83	2 p.m.		Showers.
4.	82	6 a.m.		
„	83	2 p.m.		
5.	81	6 a.m.		
„	82	2 p.m.		
6.	79	6 a.m.		Rain.
„	79	2 p.m.		
7.	80	7 a.m.		
8.	80	2 p.m.		Fair.
9.	78	7 a.m.		
„	80	2 p.m.		Id.
10.	80	7 a.m.		
„	81	2 p.m.		
11.	80	7 a.m.		
„	83	2 p.m.		Slight showers.
12.	77	7 a.m.		
„	82	2 p.m.		Id.
13.	79	7 a.m.		
„	82	2 p.m.		Fair.
29 days	82°·92 max. 79 ·36 min.	} 81°·14 med.		

[To be continued.]

XXXII. *Instructions for the Scientific Expedition to the Antarctic Regions, prepared by the President and Council of the Royal Society*.*

PHYSICS AND METEOROLOGY.

THE Council of the Royal Society are very strongly impressed with the number and importance of the desiderata in physical and meteorological science, which may wholly or in part be supplied by observations made under such highly favourable and encouraging circumstances as those afforded by the liberality of Her Majesty's Government on this occasion. While they wish therefore to omit nothing in their enumeration of those objects which appear to them deserving of attentive inquiry on sound scientific grounds, and from which consequences may be drawn of real importance, either for the settlement of disputed questions, or for the advancement of knowledge in any of its branches,—they deem it equally their duty to omit or pass lightly over several points which, although not without a certain degree of interest, may yet be regarded in the present state of science rather as matters of abstract curiosity than as affording data for strict reasoning; as well as others, which may be equally well or better elucidated by inquiries instituted at home and at leisure.

1. Terrestrial Magnetism.

The subject of most importance, beyond all question, to which the attention of Captain James Clark Ross and his officers can be turned, —and that which must be considered as, in an emphatic manner, the great scientific object of the Expedition,—is that of Terrestrial Magnetism; and this will be considered: 1st, as regards those accessions to our knowledge which may be supplied by observations to be made during the progress of the Expedition, independently of any concert with or co-operation of other observers; and 2ndly, as regards those which depend on and require such concert; and are therefore to be considered with reference to the observations about to be carried on simultaneously in the fixed magnetic observatories, ordered to be established by Her Majesty's Government with this especial view, and in the other similar observatories, both public and private, in Europe, India, and elsewhere, with which it is intended to open and maintain a correspondence.

Now it may be observed, that these two classes of observations naturally refer themselves to two chief branches into which the science of terrestrial magnetism in its present state subdivides itself, and which bear a certain analogy to the theories of the elliptic movements of the planets, and of their periodical and secular perturba-

* The President and Council having been informed by the Lords Commissioners of the Admiralty that it had been determined, in conformity with their recommendation, to send out Captain James C. Ross on an Antarctic Expedition for scientific objects, and having been requested to communicate any suggestions upon subjects to which they might wish his attention to be called, referred the consideration of each to distinct Committees, namely, those of Physics, Meteorology, Geology, Botany, and Zoology, the result of whose labours is the Report from which the above is an extract.—Ed.

tions. The first comprehends the actual distribution of the magnetic influence over the globe, at the present epoch, in its mean or average state, when the effects of temporary fluctuations are either neglected or eliminated by extending the observations over a sufficient time to neutralize their effects. The other comprises the history of all that is not permanent in the phenomena, whether it appear in the form of momentary, daily, monthly, or annual change and restoration, or in progressive changes not compensated by counter changes, but going on continually accumulating in one direction, so as in the course of many years to alter the mean amount of the quantities observed. These last-mentioned changes hold the same place, in the analogy above alluded to, with respect to the mean quantities and temporary fluctuations, that the secular variations in the planetary movements must be regarded as holding, with respect to their mean orbits on the one hand, and their perturbations of brief period on the other.

There is, however, this difference, that in the planetary theory all these varieties of effect have been satisfactorily traced up to a single cause, whereas in that of terrestrial magnetism this is so far from being demonstrably the case, that the contrary is not destitute of considerable probability. In fact, the great features of the magnetic curves, and their general displacements and changes of form over the whole surface of the earth, would seem to be the result of causes acting in the interior of the earth, and pervading its whole mass; while the annual and diurnal variations of the needle, with their train of subordinate periodical movements, may, and very probably do arise from, and correspond to electric currents produced by periodical variations of temperature at its surface, due to the sun's position above the horizon, or in the ecliptic, modified by local causes; while local or temporary electric discharges, due to thermic, chemical, or mechanical causes, acting in the higher regions of the atmosphere, and relieving themselves irregularly or at intervals, may serve to render account of those unceasing, and as they seem to us casual movements, which recent observations have placed in so conspicuous and interesting a light. The electrodynamic theory, which refers all magnetism to electric currents, is silent as to the causes of those currents, which may be various, and which only the analysis of their effects can teach us to regard as internal, superficial, or atmospheric.

It is not merely for the use of the navigator that charts, giving a general view of the lines of Magnetic Declination, Inclination, and Intensity, are necessary. Such charts, could they really be depended on, and were they in any degree complete, would be of the most eminent use to the theoretical inquirer, not only as general directions in the choice of empirical formulæ, but as powerful instruments for facilitating numerical investigation, by the choice they afford of data favourably arranged; and above all, as affording decidedly the best means of comparing any given theory with observation. In fact, upon the whole, the readiest, and beyond comparison the fairest and most effectual mode of testing the numerical applicability of a theory of terrestrial magnetism, would be, not ser-

vilely to calculate its results for given localities, however numerous, and thereby load its apparent errors with the real errors, both of observation and of local magnetism; but to compare the totality of the lines in our charts with the corresponding lines, as they result from the formulæ to be tested, when their general agreement or disagreement will not only show how far the latter truly represent the facts, but will furnish distinct indications of the modifications they require.

Unfortunately for the progress of our theories, however, we are yet very far from possessing charts even of that one element, the Declination, most useful to the navigator, which satisfy these requisites; while as respects the others (the Inclination and Intensity) the most lamentable deficiencies occur, especially in the Antarctic regions. To make good these deficiencies by the continual practice of every mode of observation appropriate to the circumstances in which the observer is placed throughout the voyage, will be one of the great objects to which attention must be directed. And first—

At sea.—We are not to expect from magnetic observations made at sea the precision of which they are susceptible on land. Nevertheless, it has been ascertained that not only the Declination, but the Inclination and Intensity can be observed, in moderate circumstances of weather and sea, with sufficient correctness, to afford most useful and valuable information, if patience be bestowed, and proper precautions adopted. The total intensity, it is ascertained, can be measured with some considerable degree of certainty by the adoption of a statical method of observation recently devised by Mr. Fox, whose instrument will be a part of the apparatus provided. And when it is recollected that but for such observations the whole of that portion of the globe which is covered by the ocean must remain for ever a blank in our charts, it will be needless further to insist on the necessity of making a daily series of magnetic observations, in all the three particulars abovementioned, whenever weather and sea will permit, an essential feature in the business of the voyage, in both ships. Magnetic observations at sea will, of course, be affected by the ship's magnetism, and this must be eliminated to obtain results of any service. To this end,

First. Every series of observations made on board should be accompanied with a notice of the direction by compass of the ship's head at the time.

Secondly. Previous to sailing, a very careful series of the apparent deviations, as shown by two compasses permanently fixed, (the one as usual, the other in a convenient position, considerably more forward in the ship,) in every position of the ship's head, as compared with the real position of the ship, should be made and recorded, with a view to attempt procuring the constants of the ship's action according to M. Poisson's theory*; and this process should be repeated on one or more convenient occasions during the voyage; and, generally, while at anchor, every opportunity should be taken of swinging round the ship's head to the four cardinal points, and executing in each position a complete series of the usual observations.

Thirdly. Wherever magnetic instruments are landed and obser-

* See Appendix A.

vations made on *terra firma*, or on ice, the opportunity should be seized of going through the regular series on ship-board with more than usual diligence and care, so as to establish by actual experiment in the only unexceptionable manner the nature and amount of the corrections due to the ship's action for that particular geographical position, and by the assemblage of all such observations to afford data for concluding them in general.

Fourthly. No change possible to be avoided should be made in the disposition of considerable masses of iron in the ships during the whole voyage; but if such change be necessary, it should be noted.

Fifthly. When crossing the magnetic line of no dip it would be desirable to go through the observation for the dip with the instrument successively placed in a series of different magnetic azimuths, by which the influence of the ship's magnetism in a vertical direction will be placed in evidence.

On land, or on ice.—As the completeness and excellence of the instruments with which the Expedition will be furnished will authorise the utmost confidence in the results obtained by Captain Ross's well-known scrupulosity and exactness in their use, the redetermination of the magnetic elements at points where they are already considered as ascertained, will be scarcely less desirable than their original determination at stations where they have never before been observed. This is the more to be insisted on, as lapse of time changes these elements in some cases with considerable rapidity; and it is therefore of great consequence that observations to be compared should be as nearly cotemporary as possible, and that data should be obtained for eliminating the effects of secular variations during short intervals of time, so as to enable us to reduce the observations of a series to a common epoch.

On the other hand it cannot be too strongly recommended, studiously to seek every opportunity of landing on points (magnetically speaking) unknown, and determining the elements of those points with all possible precision. Nor should it be neglected, whenever the slightest room for doubt subsists, to determine at the same time the geographical position of the stations of observation in latitude and longitude. When the observations are made on ice, it is needless to remark that this will be universally necessary.

With this general recommendation it will be unnecessary to enumerate particular localities. In fact, it is impossible to accumulate too many. Nor can it be doubted that in the course of antarctic exploration, many hitherto undiscovered points of land will be encountered, each of which will, of course, become available as a magnetic station, according to its accessibility and convenience.

There are certain points in the regions about to be traversed in this voyage which offer great and especial interest in a magnetic point of view. These are, first, the south magnetic pole (or poles), intending thereby the point or points in which the horizontal intensity vanishes and the needle tends vertically downwards; and secondly, the points of maximum intensity, which, to prevent the confusion arising from a double use of the word poles, we may provisionally term magnetic *foci*.

It is not to be supposed that Captain Ross, having already signalized himself by attaining the northern magnetic pole, should require any exhortation to induce him to use his endeavours to reach the southern. On the contrary, it might better become us to suggest for his consideration, that no scientific datum of this description, nor any attempt to attain very high southern latitudes, can be deemed important enough to be made a ground for exposing to *extraordinary* risk the lives of brave and valuable men. The magnetic pole, though not attained, will yet be pointed to by distinct and unequivocal indications; viz. by the approximation of the dip to 90°; and by the convergence of the magnetic meridians on all sides towards it. If such convergence be observed over any considerable region, the place of the pole may thence be deduced, though its locality may be inaccessible.

M. Gauss, from theoretical considerations, has recently assigned a probable position in lon. 146° E., lat. 66° S., to the southern magnetic pole, denying the existence of two poles of the same name, in either hemisphere, which, as he justly remarks, would entail the necessity of admitting also a third point, having some of the chief characters of such a pole intermediate between them. That this is so, may be made obvious without following out his somewhat intricate demonstration, by simply considering, that if a needle be transported from one such pole to another of the same name, it will *begin* to deviate from perpendicularity *towards* the pole it has quitted, and will end in attaining perpendicularity again, after pointing in the latter part of its progress obliquely *towards the pole to which it is moving*, a sequence of things impossible without an intermediate passage through the perpendicular direction.

It is not improbable that the point indicated by M. Gauss will prove accessible; at all events it cannot but be approachable sufficiently near to test by the convergence of meridians the truth of the indication; and as his theory gives within very moderate limits of error the true place of the northern pole, and otherwise represents the magnetic elements in every explored region with considerable approximation, it is but reasonable to recommend this as a distinct point to be decided in Captain Ross's voyages. Should the decision be in the negative, i. e. should none of the indications characterizing the near vicinity of the magnetic pole occur in that region, it will be to be sought; and a knowledge of its real locality will be one of the distinct scientific results which may be confidently hoped from this Expedition, and which can only be attained by circumnavigating the antarctic pole compass in hand.

The actual attainment of a *focus* of maximum intensity is rendered difficult by the want of some distinct character by which it can be known, previous to trial, in which direction to proceed, when after increasing to a certain point the intensity begins again to diminish. The best rule to be given, would be (supposing circumstances would permit it) on perceiving the intensity to have become nearly stationary in its amount, to turn short and pursue a course at right angles to that just before followed, when a change could not fail to occur, and indicate by its direction towards which side the focus in question were situated.

Another, and as it would appear, a better mode of conducting such a research, would be, when in the presumed neighbourhood of a focus of maximum intensity, to run down two parallels of latitude or two arcs of meridians separated by an interval of moderate extent, observing all the way in each, by which observations, when compared, the concavities of the isodynamic lines would become apparent, and perpendiculars to the chords, intersecting in or near the foci, might be drawn.

Two foci or points of maximum *total* intensity are indicated by the general course of the lines in Major Sabine's chart in the Southern Hemisphere, one about long. 140° E., lat. 47° S., the other more obscurely in long. 235° E., lat. 60° S., or thereabouts. Both these points are certainly accessible; and as the course of the Expedition will lead not far from each of them, they might be visited with advantage by a course calculated to lead directly across the isodynamic ovals surrounding them.

Pursuing the course of the isodynamic lines in the chart above mentioned, it appears that one of the two points of *minimum* total intensity, which must exist, if that chart be correct, may be looked for nearly about lat. 25° S., long. 12° W., and that the intensity at that point is probably the least which occurs over the whole globe. Now this point does not lie much out of the direct course usually pursued by vessels going to the Cape. It would therefore appear desirable to pass directly over it, were it only for the sake of determining by direct measure the least magnetic intensity at present existing on the earth, an element not unlikely to prove of importance in the further progress of theoretical investigation. Excellent opportunities will be afforded for the investigation of all these points, and for making out the true form of the isodynamic ovals of the South Atlantic, both in beating up for St. Helena, and in the passage from thence to the Cape; in the course of which, the point of least intensity will, almost of necessity, have to be crossed, or at least approached very near.

Nor is the theoretical line indicated by Gauss as dividing the northern and southern regions, in which free magnetism may be regarded as superficially distributed, undeserving of attention. That line cuts the equator in 6° east longitude, being inclined thereto (supposing it a great circle) 15°, by which quantity it recedes from the equator northward in going towards the west of the point of intersection. Observations made at points lying in the course of this line may hereafter prove to possess a value not at present contemplated.

As a theoretical datum, the horizontal intensity has been recommended by Gauss, in preference to the total, not only as being concluded from observations susceptible of great precision, but as affording immediate facilities for calculation. As it cannot now be long before the desideratum of a chart of the horizontal intensity is supplied, the maxima and minima of this element may also deserve especial inquiry, and may be ascertained in the manner above pointed out.

The maxima of horizontal intensity are at present undetermined by any direct observation. They must of necessity, however, lie in

lower magnetic latitudes than those of the total intensity, as its minima must in higher; and from such imperfect means as we have of judging, the conjectural situations of the maxima may be stated as occurring in

20° N.	80° E.	I.
7 N.	260 E.	II.
3 S.	130 E.	III.
10 S.	180 E.	IV.

Observations have been made of the horizontal intensity in the vicinities of II. and III., and are decidedly the highest which have been observed anywhere.

In general, in the choice of stations for determining the absolute values of the three magnetic elements, it should be borne in mind, that the value of each new station is directly proportional to its remoteness from those already known. Should any doubt arise, therefore, as to the greater or less eligibility of particular points, a reference to the existing magnetic maps and charts, by showing where the known points of observation are most sparingly distributed, will decide it.

For such magnetic determinations as those above contemplated, the instruments hitherto in ordinary use, with the addition of Mr. Fox's apparatus for the statical determination of the intensity, will suffice; the number of the sea observations compensating for their possible want of exactness. The determinations which belong to the second branch of our subject,—viz. those of the diurnal and other periodical variations, and of the momentary fluctuations of the magnetic forces,—require, in the present state of our knowledge, the use of those more refined instruments recently introduced. Being comparative rather than absolute, they depend in great measure (and as regards the momentary changes, wholly) on combined and simultaneous observation.

The variations to which the earth's magnetic force is subject, at a given place, may be classed under three heads, namely, 1. the *irregular* variations, or those which *apparently* observe no law; 2. the *periodical* variations, whose amount is a function of the *hour* of the day, or of the *season* of the year; and 3. the *secular* variations, which are either slowly progressive, or else return to their former values in periods of very great and unknown magnitude.

The recent discoveries connected with the *irregular* variations of the magnetic declination, have given to this class of changes a prominent interest. In the year 1818 M. Arago made, at the Observatory of Paris, a valuable and extensive series of observations on the declination changes; and M. Kupffer having about the same time undertaken a similar research at Cazan, a comparison of the results led to the discovery that the perturbations of the needle were *synchronous* at the two places, although these places differed from one another by more than forty-seven degrees of longitude. This seems to have been the first recognition of a phenomenon, which now, in the hands of Gauss and those who are labouring with him, appears likely to receive a full elucidation.

To pursue this phenomenon successfully, and to promote in other directions the theory of terrestrial magnetism, it was necessary to extend and vary the stations of observation, and to adopt at all a common plan. Such a system of simultaneous observations was organized by Von Humboldt in the year 1827. Magnetic stations were established at Berlin and Freyberg: and the Imperial Academy of Russia entering with zeal into the project, the chain of stations was carried over the whole of that colossal empire. Magnetic *houses* were erected at Petersburg and at Cazan; and magnetic instruments were placed, and regular observations commenced, at Moscow, at Sitka, at Nicolajeff in the Crimea, at Barnaoul and Nertschinsk in Siberia, and even at Pekin. The plan of observation was definitely organized in 1830; and simultaneous observations were made seven times in the year, at intervals of an hour for the space of forty-four hours.

In 1834 the illustrious Gauss turned his attention to the subject of terrestrial magnetism; and having contrived instruments which were capable of yielding results of an accuracy before unthought of in magnetic researches, he proceeded to inquire into the simultaneous movements of the horizontal needle at distant places. At the very outset of his inquiry he discovered the fact, that the synchronism of the perturbations was not confined (as had been hitherto imagined) to the larger and extraordinary changes; but that even the minutest deviation at one place of observation had its counterpart at the other. Gauss was thus led to organize a plan of simultaneous observations, not at intervals of an hour, but at the short intervals of five minutes. These were carried on through twenty-four hours six* times in the year; and magnetic stations taking part in the system were established at Altona, Augsburg, Berlin, Bonn, Brunswick, Breda, Breslau, Cassel, Copenhagen, Dublin, Freyberg, Göttingen, Greenwich, Halle, Kazan, Cracow, Leipsic, Milan, Marburg, Munich, Naples, St. Petersburg, and Upsala.

Extensive as this plan appears, there is much yet remaining to be accomplished. The stations, numerous as they are, embrace but a small portion of the earth's surface; and what is of yet more importance, none of them are situated in the neighbourhood of those *singular points* or curves on the earth's surface, where the *magnitude* of the changes may be expected to be excessive, and perhaps even their *direction* inverted. In short, a wider system of observation is required to determine whether the amount of the changes (which is found to be very different in different places) is dependent simply on the *geographical* or on the *magnetic* co-ordinates of the place; whether, in fact, the variation in that amount be due to the greater or less distance of a disturbing centre, or to the modifying effect of the mean magnetic force of the place, or to both causes acting conjointly. In another respect also, the plan of the simultaneous observations admits of a greater extension. Until lately the movements observed have been only those of the magnetic *declination*, although there can be no doubt that the *inclination* and the *intensity*

* Recently reduced to *four*.

are subject to similar perturbations. Recently, at many of the German stations, the *horizontal component* of the intensity has been observed, as well as the declination; but the determination of another element is yet required, before we are possessed of all the data necessary in this most interesting research.

The magnetic observations about to be established in the British Colonies, by the liberality of the Government, will (it is hoped) supply in a great measure these desiderata. The stations are widely scattered over the earth's surface, and are situated at points of prominent interest with regard to the Isodynamic and Isoclinal lines. The point of maximum intensity in the northern hemisphere is *in* Canada; the corresponding maximum in the southern hemisphere is *near* Van Diemen's Land; St. Helena is close to the line of *minimum intensity*; and the Cape of Good Hope is of importance on account of its southern latitude. At each observatory the changes of the *vertical component* of the magnetic force will be observed, as well as those of the *horizontal component* and *declination*; and the variations of the two components of the force being known, those of the *inclination* and of the *force* itself are readily deduced. The simultaneous observations of these three elements will be made at numerous and stated periods, and we have every reason to hope that the directors of the various European observatories will take part in the combined system.

But interesting as these phenomena are, they form but a small part of the proper business of an observatory. The *regular* changes (both periodic and secular) are no less important than the irregular; and they are certainly those by which a patient inductive inquirer would seek to ascend to general laws. Even the empirical expression of these laws cannot fail to be of the utmost value, as furnishing a correction to the absolute values of the magnetic elements, and thereby reducing them to their mean amount.

The hourly changes of the *declination* have been frequently and attentively observed; but with respect to the periodical variations of the other two elements, our information is as yet very scanty. The determination of these variations will form an important part of the duty of the magnetic observatories; and from the accuracy of which the observations are susceptible, and the extent which it is proposed to give them, there can be no doubt that a very exact knowledge of the empirical laws will be the result.

With respect to the *secular* variations, it might perhaps be doubted whether the limited time during which the observatories will be in operation is adequate to their determination. But it should be kept in mind that the monthly mean corresponding to each hour of observation will furnish a separate result; and that the number and accuracy of the results thus obtained may be such, as fully to compensate for the shortness of the interval through which they are followed. A beautiful example of such a result, deduced from three years' observation of the declination, is to be found in the first volume of Gauss's magnetical work, of which a translation is published in the fifth number of Taylor's Scientific Memoirs.

It remains to say a few words of the instrumental means which have been adopted for the attainment of these ends.

The magnetic instruments belonging to each observatory and in constant use, are, 1. a declination instrument; 2. a horizontal force magnetometer; 3. a vertical force magnetometer. These instruments are constructed after the plan adopted by Professor Lloyd in the Magnetic Observatory of Dublin. The magnet, in the two former, is a heavy bar, fifteen inches long, and upwards of a pound in weight. In the declination instrument the magnet rests in the magnetic meridian, being suspended by fibres of silk without torsion. In the horizontal force magnetometer, the magnet is supported by two parallel wires, and maintained in a position at right angles to the magnetic meridian by the torsion of their upper extremities. In both instruments the changes of position of the magnet are read off by means of an attached collimater having a divided scale in its focus. The magnetometer for the vertical force is a bar resting by knife edges on agate planes, and capable of motion therefore in the vertical plane only. This bar is loaded, so as to rest in the horizontal position in the mean state of the force; and the deviations from that position are read off by micrometers near the two extremities of the bar.

In addition to these instruments, each observatory is furnished with a dip circle, a transit with an azimuth circle, and two chronometers. Each vessel also is supplied with a similar equipment. Should therefore the ships be under the necessity of wintering in the ice,—and generally, on every occasion when the nature of the service may render it necessary to pass a considerable interval of time in any port or anchorage,—the magnetometers should be established, and observations made with all the regularity of one of the fixed observatories, and with strict attention to all the same details.

The selection of proper stations for the erection of the magnetometers, and the extent of time which can be bestowed upon each, must in a great measure depend on circumstances, which can only be appreciated after the Expedition shall have sailed. The observatory at St. Helena (the officers and instruments for which will be landed by Captain Ross,) will in all probability,—and that at the Cape (similarly circumstanced in this respect) may possibly,—be in activity by the time the ships arrive at Kerguelen's Land; which we would recommend as a very interesting station for procuring a complete and as extensive a series of corresponding observations as the necessity of a speedy arrival at Van Diemen's Land for the establishment of the fixed observatory at that point will allow; taking into consideration the possibility of obtaining during the intermediate voyage, a similar series, at some point of the coast discovered by Kemp and Biscoe. In the ulterior prosecution of the voyage, a point of especial interest for the performance of similar observations will be found in New Zealand, which, according to the sketch of the voyage laid before us by Captain Ross, will probably be visited

shortly after the establishment of the Van Diemen's Land observatory. The observations there will have especial interest, since, taken in conjunction with those simultaneously making in Van Diemen's Land, they will decide the important question, how far that exact correspondence of the momentary magnetic perturbations which has been observed in Europe, obtains in so remote a region, between places separated by a distance equal to that between the most widely distant European stations.

In the interval between quitting Van Diemen's Land and returning to it again, opportunities will no doubt occur of performing more than one other series of magnetometer observations, the locality of which may be conveniently left to the judgement of Captain Ross, bearing in mind the advantage of observing at stations as remote as possible from both Van Diemen's Land and New Zealand.

The research for the southern magnetic pole and the exploration of the antarctic seas will afford, it may be presumed, many opportunities of instituting on land hitherto unknown, or on firm ice when the vessel may be for a time blockaded, observations of this description; and in the progress of the circumnavigation, the line of coast observed or supposed to exist under the name of Graham's Land, or those of the islands in that vicinity, South Shetland, Sandwich Land, and finally on the homeward voyage the Island of Tristan d'Acunha, will afford stations each of its own particular interest.

A programme will be furnished of the days selected for simultaneous observations at the fixed observatories, and of the details to be attended to in the observations themselves as above alluded to. These days will include the *terms* or stated days of the German Magnetic Association, in which, by arrangements already existing, every European magnetic observatory is sure to be in full activity. These latter days, which occur four times in the year, will be especially interesting, as periods of magnetometrical observations by the Expedition, when the circumstances of the voyage will permit. For the determination of the existence and progress of the diurnal oscillation, in so far as that important element can be ascertained in periods of brief duration, it will be necessary to continue the observations hourly during the twenty-four for not less than one complete week. At every station where the magnetometers are observed, the absolute values of the dip, horizontal direction, and intensity will require to be ascertained.

Sydney, for a station of absolute determinations, would be with great propriety selected, as there can be no doubt of its becoming at no distant period a centre of reference for every species of local determination,

The meteorological particulars to be chiefly attended to, as a part of the magnetic observations, are those of the barometer, thermometer, wind, and especially auroras, if any. In case of the occurrence of the latter indeed, the hourly should at once be exchanged for uninterrupted observation, should that not be actually in operation. The affections of the magnetometers during thunder-storms,

if any, should be noticed, though it is at present believed that they have no influence.

During an earthquake in Siberia in 1829, the direction of the horizontal needle, carefully watched by M. Erman, was uninfluenced; should a similar opportunity occur, and circumstances permit, it should not be neglected.

Should land or secure ice be found in the neighbourhood of the magnetic pole, every attention will of course be paid to the procuring a complete and extensive series of magnetometric observations, which in such a locality would form one of the most remarkable results of the Expedition.

2. Figure of the Earth.

The Expedition being provided with invariable pendulums, with all the necessary apparatus for determining the length of the seconds pendulum, it will be highly desirable to have this important observation made at several points, especially in high southern latitudes, and generally speaking at points as remote as possible from those at which it has already been determined. The selection of these must depend on local circumstances, as regards convenience for landing the instruments and executing the operations, as well as on the times of arrival at the several points.

It would also be desirable, if a convenient opportunity occurs, to swing the pendulums on the top of some high mountain; in which case they should also be swung at the foot of the same mountain, in order to determine the difference produced by the elevation, or other effect of the high land.

Another experiment which it would be desirable to make, is to swing the pendulum on a large field of fixed ice, as far from the land as possible; and likewise on the nearest shore to such position. In all these cases more than one pendulum should be used; and at least three knife edges should be employed, in order to guard against any unforeseen anomaly that may arise.

It is scarcely necessary to state, that the direction of the line of motion, with respect to the magnetic meridian, should be noted at each station.

3. Tides.

With regard to *tides*, it is not likely that Capt. Ross's other employments will allow him to pursue observations on that subject with any continuity; nor is it desirable that he should do so, excepting he were able to carry on his observations to a much greater extent than is consistent with the nature of the Expedition. There are, however, certain objects which may be answered by occasional and detached observations, which may be briefly stated.

1. At all stations on the coasts visited, and especially at all detached islands in the middle of wide seas, it is desirable to obtain the *correct establishment* of the place, or mean lunitidal interval. This may be done with tolerable accuracy by a few observations of

successive high waters; and these must be reduced with a proper allowance for the age of the moon; that is, not only for the time of the moon's transit, but also for the seminenstrual inequality. The things to be observed are, the *mean solar* time of high water, the rise of the tide from low to high water, and, if convenient, the *mean solar* time of low water.

2. It is desirable to ascertain the existence and amount of *the diurnal inequalities* in such situations as have been spoken of. For this purpose the heights of high water should be observed for several successive tides, day and night; and, if possible, this should be done when the moon is at her greatest declination, or a few days later; at any rate, not when the moon is in the equator, nor a few days after that period.

It is very probable that a diurnal inequality of the *heights* will be detected; but it appears not worth while to attempt, under the circumstances, to detect a diurnal inequality of the *times*.

4. Meteorology.

A complete meteorological register will of course be kept in each ship during the whole continuance of the voyage; skeleton forms for the arrangement of the observed and reduced quantities are furnished. These are adapted for intervals of observation of six hours throughout the twenty-four; and although hourly observations be made, as is undoubtedly to be desired, yet the regular entry and reduction of the observations for the hours in the skeleton forms is nevertheless essential, for the sake of future comparison with those similarly entered and reduced at the fixed stations. But in considering the suggestions which it may be proper to offer upon this branch of the subject, the Council have been induced to take a more comprehensive view than might at first be supposed to be called for by the immediate objects of the Expedition. So many references have lately been made to them upon the subject of directions for meteorological observations, that they have embraced the opportunity of proposing a plan of extensive co-operation applicable alike to the Expedition, to the Magnetic Observatories about to be established, and to other Observatories for which directions have been thus solicited. The Council have therefore thought it more convenient to draw up a separate Report, which, as regards the Antarctic Expedition and the Magnetic Observatories, may be considered as supplementary to the present one.

In the way of general remark on this subject it may be observed, that it is impossible to pay too much attention to the zero points of the instruments, especially the barometer. Every thermometer and barometer, furnished both to the ships and for the observatories, will in the first instance have been carefully compared with those of the Royal Society; and one barometer in each ship should be continually referred to as a standard, whenever the instruments are landed and on their return on board, so as to detect and take account of any change which may have occurred in the interval: while the two

standards, whenever the ships are in company, will become checks on each other through the medium of the register. Nor should the opportunity be lost of comparing these standard barometers (by the intervention of portable ones) with the standard barometer of the Cape Observatory, and with that used at Port Arthur, Van Diemen's Land, in the meteorological register kept by Sir J. Franklin's orders by Mr. Lempriere, as well as with the standard at the observatory at Paramatta, and with any other instrument likely to be referred to as a standard or employed in research elsewhere.

The general fact that the barometer at the level of the sea does not indicate a mean atmospheric pressure of equal amount in all parts of the earth,—but, on the contrary, that the equatorial pressure is uniformly less in its mean amount than that at and beyond the tropics,—was first noticed by Von Humboldt, and has since been demonstrated by the assemblage of many observations made during voyages and on land by Schouw, as well as by other observations, an account of which will be found in the Reports of the Meteorological Committee of the South African Philosophical Society for 1836 and 1837. This inequality of mean pressure is a meteorological phenomenon of the greatest and most universal influence, as it is, in fact, no other than a *direct measure of the moving force*, by which the great currents of the trade-winds are produced; so that the measure of its amount, and the laws of its geographical distribution, lie at the root of the theory of these winds. The progress of barometric depression on approaching the line, and re-ascension in receding from it, will therefore be watched with interest proportionate to its intrinsic importance during the voyage outwards and homewards.

But it may very well happen that phenomena purely local, of the same nature, may exist, not as *cause* but as *effect*; in other words, that the regular currents once established may, in particular localities, determined by the configuration of continents and by the influence of oceanic currents, or other causes, form permanent eddies or atmospheric ripples, so to speak, under which the mean pressure may deviate materially from the general average. An instance of permanent barometric depression of this kind, in the neighbourhood of the sea of Ochotzk, is mentioned by Erman; and a second seems to be pointed out in the neighbourhood of Cape Horn, by some remarks stated to have originated with Captain Foster; and it is not impossible that something of the same kind, but of an inverse character, may be found to obtain in that remarkable district of Siberia mentioned by Erman, where during winter clouds are unknown and snow never falls; and it is somewhat curious to notice that the localities in question are not far from antipodes to each other.

In the outward and homeward passages of the Expedition across the equator (especially should the ships be delayed by calms), opportunity will be presented of determining the amount of diurnal barometric fluctuation, apart from the interfering influence of land and sea breezes, or their equivalents far inland, which in all land observations encumber and disturb this somewhat obscure pheno-

menon; as well as for ascertaining, also apart from those influences, the existence or non-existence of that difference between the diurnal and nocturnal maxima and minima, which has been proved to exist in some localities, and surmised to be general*.

Connected with the equatorial barometric depression, and the ascensional current of heated air which produces that depression, is a phenomenon which may serve to elucidate the mechanism of this current in its origin, as well as to illustrate the mode in which ascending currents occasionally produce rain. It cannot be supposed that the whole body of the equatorial atmosphere rises *en masse*, or with any regularity or steadiness. Such a movement would be out of analogy with what we know of the movements of fluids in general. Its *tendency* to rise is general, but this tendency is diverted by a thousand local influences, and concentred on particular points, where it results in ascending columns and sheets, between which windflaws, capricious in their direction and intensity, and often amounting to sharp squalls, mark out the course of their feeders and of the indraft of cooler air from a distance to supply their void. Now the existence of such ascending columns is rendered frequently visible in a very unequivocal way, by vast piled-up masses of cloud of that peculiar form which has been called *cumulostratus*, the bottom being flat and ill-defined, the upper parts towering to an immense height, and ragged with great protuberances. From the bases of these great cumular piles are almost constantly seen to descend those violent showers so common in the calm latitudes.

It would be interesting on many accounts to obtain *measures*, even if somewhat vague, of the altitudes at which the *bases* of these clouds rest, as well as of the height of their summits, and to measure the temperature of the rain which falls from them at successive periods, as they pass over the ship, so as to ascertain whether the rain which falls along their axis be not colder (from coming at least in part from a greater elevation) than that from their skirts. The vapour plane, in such circumstances being nearly or precisely uniform over vast tracts of sea, the altitude of the base of such cloud vertically overhead may be considered the same as that of any other favourably situated for measure. In fact the determination of the *mean height of the vapour plane* at and near the equator is one of high meteorological import, and is connected by no circuitous steps with all the most interesting questions regarding the distribution of aqueous vapour over the globe and the irrigation of the continents.

5. Distribution of Temperature in the Sea and Land.

Connected as this subject is with meteorology, it requires in some points of view to be considered apart. As the currents in the atmosphere are produced by the difference of temperature in its polar and equatorial regions, so it may be contended are those of the ocean by differences of temperature due to the same geographical causes. Such

* See Reports of Met. Com. S. A. Phil. Soc. above referred to.

is the view taken by M. Arago in his elaborate instructions for the voyage of the *Bonite*, and it would appear undoubtedly more just than that which attributes them *wholly* to the friction and pressure of the winds. Nevertheless it must not be forgotten that there is an essential difference in the modes of action in the two cases. The sun's heat is effective in heating the air mainly *from below*, where it is in contact with the earth or water which absorb the rays and communicate them to the air above. In the sea the case is otherwise. The sun's rays are totally absorbed at the surface, and no ray reaches the bottom of any sea deserving the name. No deep stratum of water, therefore, can be permanently maintained by the sun's *direct* heat at a temperature greatly above what it would have independently of its direct action. Hence the motive power in a system of currents so originating must be sought, not in the *ascensional* force at the equator, but in a *descensional* one in the polar regions, or rather in that one polar region in which winter prevails. The order of the phenomena then is precisely the reverse of what obtains in the atmosphere; moreover the seat of the efficient agency is not only much less extensive than in the case of the atmospheric currents, but also subject to a semiannual shifting from one to the other extremity of the earth's axis, both which causes must tend greatly to diminish the average energy of the effect.

Practically speaking, the question resolves itself into one of fact, which observation only can decide. Is there in the whole column of water between the surface of the ocean and its bed at the poles, as compared with a column of equal depth at the equator and *in free communication with it*, a descensional power or not? and what is its amount? These questions can only be resolved by observations of the temperature and saltness of the sea, at various and considerable depths, in different latitudes, and under a great variety of local circumstances. The procuring such observations, and the preservation of specimens of the water, or the determination on the spot of their specific gravities, will afford a useful occupation in calms, and may be recommended as well worthy of attention. Theoretically speaking, the subject is more complicated than at first appears, since it cannot but be that some considerable portion of solar heat absorbed by the equatorial continents,—in place of finding its way out of the earth by radiation at the poles, in the mode of subterraneous communication suggested by Fourrier,—must escape through the bed of the ocean into its waters, and so be carried into their circulation.

Opportunities for determining the temperature of the ocean at great depths must of course be rare; but at moderate depths it can always be done with comparatively little trouble, and we would, therefore, suggest the propriety of making observations of this element at two moderate and constant depths (say 150 and 300 fathoms), by the aid of a self-registering thermometer attached to a sounding line whenever the ship's way shall be such as to allow their being made with precision.

6. Currents of the Ocean.

These are either subaqueous or superficial, and, like those in the atmosphere, both may coexist at the same place, with different directions and velocities. Of the former we know almost nothing, and of the latter but little compared with what would be desirable and most useful. The practice of daily throwing overboard a bottle corked and sealed with the latitude and longitude of the ship at noon ought not to be neglected. A single instance of such a record being found may suffice to afford indications of the utmost value, while the trouble and cost are too trifling to mention.

As no sea can be supposed absolutely motionless, the presence of a shoal, by casting up at the surface water which, but for it, would have continued to sweep along at a greatly lower level with the general body of the current, must bring the temperature of the surface water into nearer correspondence with that below. In low latitudes the surface water is hotter than that below; and accordingly it is a general remark, that the temperature sinks as the water shoals, or even in passing over banks whose depth is very considerable. If this theory of the phenomenon be correct, the contrary ought to be observed in situations where the surface water is colder than that below, as it is known to be under particular circumstances in the Polar Seas. In still larger tracts in high latitudes the seas have nearly a uniform temperature throughout their whole depth. In such circumstances should any superficial variation of temperature be observed in passing over a shoal or bank, it could only be ascribed to radiation. The subject is one of considerable interest to the navigator, as the approach to land or to shoal water is indicated by the thermometer with a high degree of sensibility. We have before us recent observations of this kind, the one at entering Table Bay in 1834, the other at quitting it in the present year. In the former case the temperature fell 9° Fahr. in passing from deep water into the Bay; in the latter under reverse circumstances a rise of no less than 13° Fahr. was experienced, the temperature of the air remaining unaltered. The last-mentioned observations being very remarkable, the particulars are annexed.

Memorandum of observations made on board the Earl of Hardwicke, H.E.I.C.S. by Captain Henning.

March 17th, 1839. Temperature of air at 5^{h} P.M., four miles from Cape Town .... 64°·0
Of Sea .... 52°·0
5^{h} 30^{m} One mile north of Robben Island. Air .... 64°·0
Sea .... 57°·0
March 18th at sea. Air .... 64°·0
Sea .... 65°·0

The opportunity of re-examining this point, and in general, of investigating more closely the phenomena of temperature in the neigh-

bourhood of the Lagullas Bank, will, of course, not be lost sight of as the ships approach to and leave the Cape.

The distribution of temperature over the globe must greatly depend on the intensity which the solar rays possess on attaining the surface of the earth after traversing the atmosphere in different latitudes. To subject this point to direct inquiry in a mode which after many years' trial has been found to give very satisfactory results, Actinometers are provided, and accompanied with very precise directions for their use. They should be observed only when the sky in the immediate neighbourhood of the sun is perfectly free from visible cloud. On the other hand, depression of temperature caused by diurnal and nocturnal radiation by the only means we at present possess for that purpose,—viz. that of thermometers blackened and exposed in reflectors to the sky,—will form a useful and valuable supplement to the actinometric researches. With a view to the collection of facts illustrative of the distribution of temperature on land, wherever the ships may touch with a prospect of remaining some days, no time should be lost, on landing, in burying in the earth one or more bottles (filled with spirits, if there should be danger of water freezing), well packed in cases, or boxes stuffed with non-conducting matter, such as woollen cloth, pounded charcoal, &c., but so as to leave easy access to the neck, which should be wide enough to introduce the bulb and stem of a good thermometer, so as to take the temperature of the contained liquid rapidly, before it can have become altered by exposure in the air on taking up the bottle. Bottles so arranged should be buried at depths of three, six, nine, twelve, &c. feet*, according to the facilities of penetrating the soil, and abandoned till the time of departure, so as to ensure their acquiring the precise temperature of the soil; and when taken up should immediately have the temperature of the included liquids ascertained. In case of very prolonged sojourn, monthly readings should be taken. The temperatures of all springs and wells should also be diligently noted and registered.

Connected with the transcalescence of the air, is the transparency of the sea. The stimulus of the solar light no doubt affects the surface of mollusca at great depths, and numerous points of physical inquiry would be elucidated if we knew the co-efficients of extinction of the solar rays by pure sea water. As far as the luminous rays are concerned (or at least the chemical), the actual intensity of these rays at various depths might be very easily ascertained, both for direct sunshine and that of cloudy daylight, by the aid of Mr. Talbot's sensitive paper; which, duly guarded from wet by varnish and interposition between glass plates, might be sunk, face upwards in a small frame, while a portion of the same paper, cut from the same sheet, should be similarly exposed on deck, and partially shaded, inch by inch, from minute to minute, (or for a smaller interval according to the sensitiveness of the paper) with a view to immediate comparison, between decks by a light not strong enough to alter the tint.

A simple and convenient mode of photometric measurement is also

* These are the depths adopted in Mr. Forbes's recent experiments.

furnished by the sensitive paper above alluded to, by the exposure of a small portion of it to the sun at noon for a given time, suppose ten seconds, and subsequent comparison with a scale of tints. Paper duly prepared for these purposes will be supplied for the use of the expedition. During solar eclipses such paper ought to be exposed at intervals of five minutes.

The temperature of the soil under the direct influence of the sun as indicated by a thermometer barely covered with dry earth, is an element of importance to the botanist, and may be recommended as an apt accompaniment to actinometric observations. The thermometer used should have a scale reading at least to 180° Fahr.

The height of the line of perpetual snow, by whatever indications marked, should also be ascertained, wherever practicable.

7. Depth of the Sea.

Soundings to as great a depth as practicable should be taken wherever opportunities may offer. Great difficulty, however, is well known to exist in the way of procuring any exact result, or indeed any result at all in very deep seas; and various methods (all objectionable) have been proposed and tried. Could any means be provided to keep out the water from a shell, and at the same time ensure its explosion on striking the bottom, the time elapsed, between casting the shell overboard and hearing the explosion, would indicate the depth with great precision; nor need we fear that, if the explosion took place, the sound would not be heard, sound being propagated through water with infinitely greater sharpness and clearness than through air. To overcome the enormous external pressure, and to enable the charge to burst the shell, it is probable that mere gunpowder might not suffice. Should this be apprehended, a mixture of fulminating mercury with the charge in about equal proportions, would probably effect the object. At least we know, from experience, the vast increase of bursting power which is communicated to powder by such addition. It has also been suggested that an *echo* from the bed of the ocean might be heard, were a shell exploded just beneath the surface (as an echo from the earth is heard in the car of a balloon); and attempts, though imperfect ones, have been made to subject this proposal to trial, the reason of the failure of which does not very distinctly appear. The maximum depth of the sea is a geological datum of such value, that a few failures incurred in attempts may very well be tolerated when placed in competition with the interest of even partial success.

8. Atmospherical Phenomena.

There can be little need to call the attention of navigators to anything relating to winds, storms, lightning, &c.; yet there are some points to which attention may be expressly drawn, viz. to such distribution and movements of the clouds as indicate the existence at the same time of an upper and an under current of wind

moving in opposed or differing directions. In such cases, the sun, moon, or a star should be taken as a point to fix the eye. In storms the barometer should be very assiduously noted in relation to the varying phases of the gale and the changes of wind, and particularly to those sudden shifts of wind which characterize revolving storms. The Council are not aware that the state of the barometer during "a white squall" has ever been very carefully noted from instant to instant; or that it, or the more sensitive sympiesometer, have been referred to during the approach and recess of a waterspout.

The phenomena of ordinary thunder-storms may be thought to afford little matter for remark, and extraordinary ones will be noted of course. Yet there is one point to which we should wish that some attention might be paid,—it is the sudden gush of rain which is almost sure to succeed a violent detonation immediately over-head. Is this rain a *cause* or a *consequence* of the electric discharge? Opinion would seem to lean to the latter side, or rather, we are not aware that the former has been maintained or even suggested. Yet it is very defensible. In the sudden agglomeration of many minute and feebly electrified globules into one rain drop, the quantity of electricity is increased in a greater proportion than the surface over which (according to the laws of electric distribution) it is spread. Its tension therefore is increased, and may attain the point when it is capable of separating from the *drop* to seek the surface of the *cloud*, or of the newly-formed descending body of rain, which, under such circumstances, and with respect to electricity of such a tension, may be regarded as a conducting medium. Arrived at this surface, the tension for the same reason becomes enormous, and a flash escapes.

The following points should be observed, with a view to this mode of regarding the formation of lightning. 1st. The actual electric state of *that* rain which follows suddenly after a discharge originating vertically over head.

2nd. Does lightning ever happen without rain *in the immediate point where it originates*, or at least without a rapid formation and increase of cloud at that point?

3rd. Does it ever lighten from a cloud undergoing actual diminution from evaporation?

4th. Do the cumular clouds, already noticed as continually forming and raining in the calm latitudes, usually or frequently send forth flashes of lightning; and if so, under what conditions, and with what effects?

Observations of Aurora will form a highly interesting subject, should the Expedition be under the necessity of wintering, or of passing any of the later part of the season in south latitudes admitting of their exhibition. Their effects on the magnetic needle will of course be narrowly watched; but all their phenomena should be minutely registered, such as the formation, colour, extent, situation, movement, and disappearance of arches, patches, banks, and streamers. In particular we would draw attention to an appearance which sometimes occurs, and which cannot but be regarded as highly instructive. It consists in *pulsations* propagated with more or less swift-

ness through *patches* of sky *of definite forms*, which however become visible only in successive portions, as the pulse traverses them, giving the idea of masses of vaporous matter not visible *per se*, but rendered fitfully so, either by a band of light cast in succession over every part of them from without, or by a temporary phosphorescence developed within their substance when traversed by electric matter. Such pulsations as above described formed very remarkable features of the auroras of October 12, 1833, and of January 18, 1839.

Any indication of the *near vicinity* of auroral phenomena, or of their existence at a level below that of ordinary clouds, should be most minutely investigated at the moment, and carefully and circumstantially recorded.

On the nights from the 11th to the 14th of November the sky should be watched for the periodical meteors, whose existence seems now to be placed out of doubt; as also from the 9th to the 13th of August; and in general any remarkable display of shooting stars should be noted. The zodiacal light also should be observed in clear nights, with a view to the better defining its limits, and ascertaining if it be really, as some have supposed, variable in its extent or lustre. Remarkable halos, parhelia, and other atmospheric phenomena, should be recorded, and careful measures of their dimensions taken with sextants or other instruments.

9. Variable Stars.

During night-watches in clear weather, in southern regions, many interesting observations might undoubtedly be made by any one acquainted with the constellations, or provided with a celestial map, as to the comparative lustre and variability of stars. Especially we would point out to the attention of such an observer the stars α Hydræ et Crateri as certainly, and δ Orionis as probably, variable; the former at its greatest brightness being equal to ϵ Argûs, and at its least equal or somewhat inferior to δ Argûs, which are the best stars for comparison with it. Its period of change, however, being only very imperfectly known, additional observations would be valuable. The remarkable star η Argûs should also be compared with others of the same apparent brightness, or nearly so, with a view to continue the history of its late extraordinary change of lustre. And we would earnestly recommend to any one who may undertake such observations, to form a list of a certain moderate number of stars, graduating from the first magnitude downwards by almost insensible steps, and having rendered himself familiar with them, to note their arrangement in order of brightness,—not once only, but on a great many nights, forming on each occasion separate independent judgements; trusting on no account to any printed catalogue, and diligently recording and preserving his memoranda. Such observations are not part of the ordinary business of astronomical observations, and are therefore neglected and abandoned to the amateur, the traveller, or the seaman in his night-watches, which they will be found to beguile

of much of their tedium, and to reward by the frequent detection of variable stars not previously recognized as such.

10. Refraction.

The determination of refractions near the horizon, both of celestial and terrestrial objects in high southern latitudes, will form a very interesting subject of study. They may be pursued in various modes, of which perhaps the easiest is to note the disappearance of particular stars behind the horizontal edge of a board, erected at some considerable distance from a fixed point of observation, and then to ascertain, with all precision, the altitude of the line of disappearance, accompanying such observation with the height of the barometer and thermometer. Vertical diameters of the sun or moon, when very near the horizon, with the corresponding altitudes, will also be of use, as well as measurements of the distances of two considerable stars on the same vertical, and direct measures of the altitudes of one and the same star in the progress of its diurnal course when near the horizon. The curve of terrestrial refraction might also be actually traced out by a leveling-staff. Any cases of unusual refraction, mirage, reduplication and inversion of images, and of lateral refraction, should be recorded.

11. Eclipses.

In annular or total solar eclipses the optical circumstances attending the formation and rupture of the ring should be minutely attended to, as well as the defalcation of light and heat, to be measured by their appropriate methods, as detailed in the Meteorological instructions.

The solar eclipses of $\left\{\begin{array}{ll}\text{January} & \text{11, 1842} \\ \text{June} & \text{27, 1843}\end{array}\right.$

may possibly be central, or very large in some part of the progress of the Expedition. In lunar *total eclipses* the occultations of stars, whether large or small, should be looked for, and any apparent projection on the disk noticed. Great attention should also be paid to the intensity, colour, and distribution of illumination over the disk during the *total* eclipse, as indicative of the general state of *the earth's* atmosphere in that great circle of the globe which at the moment is at right angles to the visual ray.

As a summary of this Report, the Council recommend to the Admiralty to give instructions for the making and recording of the following observations, experiments, and researches.

1. Magnetic observations of the inclination, declination, and intensity at sea, throughout the voyage, daily in both ships, whenever the motion of the vessel will permit.
2. Precise determinations of the same particulars wherever the Expedition may land, or disembark on ice.
3. Most careful series of magnetometric observations, in corre-

spondence with those to be made at the fixed observatories, according to a plan concerted with the officers of those observatories, and with Professor Lloyd, the particulars of which will be furnished to each party concerned, and distributed to all the European and other observatories.

4. A circumnavigation of the Antarctic Pole, with a view to affording opportunities and proper stations for magnetic and other observations.

5. An inquiry into the actual position of the southern magnetic pole or poles, and the points or foci of greatest and least total and horizontal intensity, and into the course and figure of the isodynamic ovals presumed to occupy the area of the South Atlantic.

6. The determination of the length of the invariable pendulum at several stations in high south latitudes.

7. Observations of the tides, i. e. of the heights and times of high water, made at such stations at which the ships may remain long enough, and at which the correct establishment is unknown.

8. The keeping of a regular meteorological register in both ships during the whole voyage, and the paying attention to the phenomena of solar and terrestrial radiation, and generally to all phenomena bearing on the subject of meteorology.

9. The temperature of the sea at the surface and at stated moderate depths should be observed as frequently as possible, and whenever opportunity may occur, also at the greatest depths attainable; and attention should be directed to the temperature of currents and shoals, as well as to its variation on approaching land. The temperature of the soil at various depths should be taken on landing, as well as that of springs, wells, &c.

10. Soundings should be attempted in deep seas, and specimens of the water brought up be preserved for future examination.

11. Observations should be collected of the aurora in high south latitudes; and attention directed to meteors and shooting stars on those occasions when experience has shown that they occur periodically in great abundance; as well as to the appearance of the zodiacal light, and other phenomena of a similar occasional nature.

12. Observations of the comparative brightness of southern stars should be procured, and especially of the variable stars α Hydræ and η Argûs.

13. The amount and laws of horizontal refraction, both celestial and terrestrial, in high south latitudes, should be investigated.

14. The phenomena of eclipses should be attended to.

INSTRUCTIONS FOR MAKING METEOROLOGICAL OBSERVATIONS.

The Council of the Royal Society, while they have been occupied in preparing instructions for making meteorological observations at the fixed magnetic observatories about to be established by the Government at Montreal, St. Helena, the Cape of Good Hope, Van Diemen's Land, and the different stations to be visited by the Antarctic Expedition under Captain James Clark Ross, and in reporting on various references made to them of applications for instructions for similar observations by the Secretary of State for the Colonies, the Honourable Court of Directors of the East India Company, and the Corporation of the Trinity House, have availed themselves of this opportunity for proposing a plan of extensive co-operation, the general adoption of which by observers cannot fail to produce the most advantageous results to meteorological science.

After maturely considering the subject, they do not presume to anticipate that what they may suggest will not be liable to objections, for their object will be to include within their compass many excellent series of observations which are already in progress, rather than to propose a degree of theoretical perfection, the attainment of which the present state of the science may not perhaps admit of. Systematic co-operation is the essential point to which at present every thing else should be sacrificed; and co-operation on almost any plan would most certainly be followed by more beneficial results than any number of independent observations, however perfect they might be in themselves.

The plan of co-operation should, in fact, be regarded at present as merely temporary and preparatory; but if steadily adhered to for a few years, it would certainly furnish the most perfect data for its own correction, which could then from time to time be applied with facility and precision.

The Council are not without hopes that amateurs of science may be induced to conform to these suggestions, even at the temporary sacrifice of their own views and convenience; for no one can reflect on the immense amount of labour which is now rendered useless for want of the requisite uniformity and precision, without being convinced of the necessity of remedying an evil which has already been of too long standing, and continues to be a reproach to science. Many, of course, will not have it in their power to fill up the plan in all its details; but they will contribute greatly to forward the design, if, in such observations as they may find it convenient to make, they strictly comply with the rules proposed. They will be further encouraged to lend their aid to a comprehensive system, by the consideration that it will be adopted by the Government Observatories, as well as by those about to be established by the East India Company, and will of course be acted upon in the comparison and discussion

of the observations made at these institutions by the scientific authorities who will be entrusted with the execution of this task.

The suggestions which the Council wish to offer will relate, 1st, to the times of observation; 2ndly, to the situation of the instruments to be observed; 3rdly, to the correction of the observations; 4thly, to a form of registry, which may place many of the results in a striking point of view, and facilitate comparisons.

1. Barometers.

Times of observation.—The purposes of meteorological observations would be most perfectly and most expeditiously obtained by hourly observations throughout the year; but as at present such a course of unremitting labour cannot be hoped for, it is necessary to select periods, at longer intervals, calculated to embrace the extremes of the periodical oscillations to which the pressure of the atmosphere is subject, and to ensure that uniformity of system at different stations on which the value of such observations so much depends. It is probable that the hours of 3 A.M., 9 A.M., 3 P.M., and 9 P.M., nearly coincide with the daily maxima and minima of the barometric column at the level of the sea, over a large portion of the globe; and it is desirable that as extensive a comparison as possible should be instituted at these hours.

It is not, however, too much to expect that in regular observatories hourly observations should be made, for 24 hours, once in every month; and when this cannot be effected, it is of the utmost importance that they should be made at least four times in the year, namely, at the summer and winter solstices, and at the spring and autumn equinoxes. One of the results of these hourly observations would probably be the indication of the exact times of the daily maxima and minima of pressure at different stations, which, if not found to coincide with the hours provisionally adopted, might ultimately be substituted for them under future directions.

Hourly observations at the equinoxes and solstices have been already instituted at numerous points both of Europe and America, at the suggestion of Sir John Herschel, whose directions should be strictly attended to. They are as follows:—

The days fixed upon for these observations are the 21st of March, the 21st of June, the 21st of September, and the 21st of December, being those, or immediately adjoining to those, of the equinoxes and solstices in which the solar influence is either stationary or in a state of most rapid variation. *But should any one of those 21st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the 22nd.* The observation at each station should commence at 6 o'clock A.M. of the appointed days, and terminate at 6 A.M. of the days following, according to the usual reckoning of time at the place.

The commencement of each hour should be chosen, and every such series of observations accompanied by a notice of the means used to obtain the time, and, when practicable, by some observation

of an astronomical nature by which the time can be ascertained within a minute or two.

The Council now propose to extend these observations in regular series to the 21st of every month, with the same reservation with regard to Sundays.

It is to be hoped that in regular meteorological observations the six-hourly observations may not be found to be impracticable throughout the year; but in any case where it may be impossible to observe regularly at 3 A.M., an effort should be made to include the hour on the days of the new and full moon, and quadratures, or at least on the days of the new and full moon;—as it must be borne in mind, that in what concerns the great meteorological questions on which the most interesting features of the subject depend, the night is quite as important as the day, and has been hitherto far too much neglected.

Whatever hours, however, may be selected for the regular series of observations, the greatest care should be taken not to substitute or interpolate in an irregular manner observations at any other hours.

It is much to be wished that occasional observations may be made under remarkable circumstances, such as during great rises or great falls of the barometer, at the period of great storms, earthquakes, &c.; but such observations should be registered apart.

The barometer should be placed in an apartment subject to as little variation of temperature as possible, and in a good light; and to facilitate night observations, an arrangement should be made for placing behind it a light screened by a sheet of white paper, or other diaphanous substance. Great care should be taken to fix it in a perpendicular position by the plumb-line. Its height must be carefully ascertained above some permanent and easily-recoverable mark, either in the building in which it is situated, or in some more permanent building, or rock, in its immediate vicinity; and no pains should be spared to ascertain the relation which such mark may bear to the level of high and of low water at spring tides, and ultimately to the mean level of the sea.

Changes in the adjustments of meteorological instruments should be most carefully avoided; but whenever any alteration may be absolutely necessary, they should be made with all deliberation, scrupulously noticed in the register, and the exact amount of the change thence arising in the reading of the instrument under re-adjustment ascertained. As far as possible, registers of meteorological observations should be complete; but if, by unavoidable circumstances of absence, or from other causes, blanks occur, no attempts to fill them up by general recollection, or by the apparent course of the numbers before and after, should ever be made.

The observatories established by the Government are furnished with two barometers each, of Newman's construction—the one a standard, and the other portable; and they are accompanied by accurate directions for fixing and observing them.

The standard instrument is of large dimensions, its tube being of the diameter of 0·6 inch. It requires two adjustments: 1st, The whole scale, which is of brass, is moveable, and terminates in an

ivory point, which is carefully brought down to the surface of the mercury in the cistern, and the two are known to be accurately in contact when the actual point and its reflexion appear just to touch one another. The scale is laid off from this point from an authentic standard, at the temperature of 32°.

2nd. The second adjustment is that of the vernier, in which the upper part of the scale terminates, to the surface of the mercury in the tube. For this, both the back and front edge are made to coincide, and brought down so as to form a tangent to the curve, and just to exclude the light between them at the point of contact. In making both these adjustments, it is desirable that the eye should be assisted by a magnifying glass. Before the observation is made the instrument should be slightly tapped, to free the mercury from any adhesion to the glass; but any violent oscillation should be avoided.

The portable barometer has only one adjustment, namely, that of the vernier to the upper surface of the mercury in the tube, which adjustment must be effected with the same precaution as in the case of the standard instrument.

This first reading may be entered in the column prepared for it in the register, and beside it the temperature of the mercury carefully read off from the thermometer which dips into the cistern.

As, in the case of the standard barometer, the first measure is taken immediately from the surface of the mercury in the cistern, it requires no correction for the different capacities of the tube and cistern. Neither does it require any correction for capillary action, as the large diameter of the tube renders this correction inappreciable.

The portable barometer, however, requires corrections for both these circumstances. For the purpose of the former, the *neutral point* is marked upon each instrument, or that particular height which, in the construction of the instrument, has been actually measured from the surface of the mercury in the cistern.

It is obvious that, in almost every case, the mercury will stand either above or below the neutral point: if above, a portion of the mercury must have left the cistern to enter the tube, and consequently must have lowered the surface in the cistern; if below, a quantity of mercury must have left the tube, and, entering the cistern, raised the level of the mercury in it. For the correction of observations for this circumstance, the relation of the capacities of the tube and cistern have been experimentally ascertained, and are marked upon the instrument: thus, *capacity* $\frac{1}{50}$th, indicates that for every inch of elevation of the mercury in the tube, that in the cistern will be depressed one 50th of an inch. Thus, when the mercury in the tube is above the neutral point, the difference between it and the neutral point is to be divided by the capacity, and the quotient being added to the observed height, the result will be the corrected height. Or if the mercury at the time of observation should be below the neutral point, the difference of the two is to be divided as before, and the quotient to be subtracted from the observed height. Thus, suppose the capacity to be $\frac{1}{30}$th, the neutral point 30 inches, and the observed

height 30·500 inches, the difference is 0·5 inch, which divided by 50 gives 0·01 inch to be added to the observed height, producing 30·51, the corrected height; or if the observed height be 29 inches, the difference, 1 inch, divided by 50, gives ·02 inch to be subtracted from the observed height, giving 28·980 inches for the corrected height.

The second correction required is for the capillary action of the tube, the effect of which is constantly to depress the mercury in the tube by a certain quantity inversely proportioned to the diameter of the tube. In the instruments furnished to the fixed observatories the amount has been experimentally determined during their construction, and marked upon the instrument; the quantity is always to be added to the height of the mercurial column, previously corrected as before. For the convenience of those who may have barometers, the capillary action of which has not been so determined, a table of the corrections for tubes of different diameters is placed in the appendix.

The marine barometers furnished to the Antarctic Expedition differ in nothing from the other portable barometers but in the mode of their suspension, and the necessary contraction of the tubes to prevent oscillation from the motion of the ship, and require the same corrections.

When these two corrections have been made in the first reading of the portable barometer, it should agree with the direct observation of the standard barometer; and it is very desirable that frequent comparative observations should be made of the two instruments, in order to ascertain whether there may be any permanent difference between them. Should this be the case, the amount may be marked upon the instrument, and allowed for as an index error, in order that, if an accident should happen to one, the other may be substituted for it without detriment to the regular series of observations.

It is to be presumed that the portable barometer will frequently be employed in ascertaining the altitude of remarkable points in the vicinity of the observatories, or of the more permanent stations of the Expedition.

The instruments furnished to the observatories have been all independently graduated and compared with the standard of the Royal Society; and in all cases it is desirable that such a comparison should be made with some standard instrument of authority, directly, or by means of a good portable barometer. In making such comparisons, all that is necessary is to record five or ten simultaneous readings of both instruments, deliberately made, at intervals of a few minutes from each other, after at least an hour's quiet exposure, side by side, that they may have the same temperature. If compared by two observers, each should read off his own barometer in his usual manner, then each should verify the other's result. By this means the zero of one standard may be transported over all the world, and that of others compared with it ascertained. To do so, however, with perfect effect requires the utmost care in the transport of the intermediate barometer, and is by no means an operation either of trifling import or

of hurried or negligent performance: some of the greatest questions in meteorology depend on its due execution.

The next correction, and in some respects the most important of all, is that due to the temperature of the mercury in the barometer tube at the time of observation. To obtain this every barometer requires to have attached to it a thermometer, which in the instruments furnished to the observatories dips into the mercury in the cistern, and this must be read and registered at each observation of the barometer. In the appendix will be found a table calculated by Professor Schumacher, which gives for every degree of the thermometer and every half inch of the barometer, the proper quantity to be added or subtracted for the reduction of the observed height to the standard temperature of 32° Fahr.

It must, however, be observed, that this table is only calculated for barometers whose scales are engraven upon a rod or plate of brass reaching from the level of the mercury to the vernier. In many barometers the scale is engraved upon a short plate of brass fixed upon the wooden frame of the instrument, and the compound expansion of the two substances can only be guessed at, but must be obviously less than if the whole length had been of brass. As a near approximation for such imperfect instruments, another table has been placed in the appendix, in which the lesser expansion of glass has been substituted for that of brass. No scientific observer, however, would willingly use such an instrument.

Although all these corrections are necessary for the strict *reduction* of registered observations, they ought not to be applied to individual observations previously to registry. In the blank forms of register furnished to the observatories, one sheet is devoted to uncorrected observations, and a second to the corrected; and it is much to be wished that the proper reductions should be made as soon after the observations as possible.

2. Thermometers.

Times of observation.—The external standard thermometer should be observed and registered at the same times as the barometer, and all the register thermometers may be read off at the time of the 9 A.M. observation, and their indices re-adjusted. But as double maxima frequently, and double minima occasionally, occur, in consequence of sudden changes of temperature, both the thermometers should be occasionally inspected with a view to ascertain whether the motion of either the mercury or the spirit has been reversed in an unusual manner; and such double maxima or minima should be recorded apart as *supernumerary*, with the dates and leading features of the case.

Each observatory has been furnished with a standard thermometer, of which duplicates have been deposited at the Royal Society, and which have been carefully compared with an authentic standard. With this standard it is recommended that all other thermometers be carefully and frequently compared, and their differences,

at one or more temperatures (the wider asunder the better), marked upon their scales and applied as index errors. This is particularly necessary with the register thermometers, whose construction renders them most liable to such errors.

In placing the standard thermometer, an exposure should be chosen perfectly shaded from the sun, one where no reflected sunbeams from water, buildings, rocks, or dry soil can reach it, and one which is easily accessible for observation. It should be *fixed*, not merely *hung*, upon a bracket projecting six inches from the wall, or other support to which it may be attached, and it must be *completely* sheltered from rain by a screen, so that the bulb shall never be wetted. In reading it, the observer should avoid touching, breathing on, or in any way warming it by near approach of his person; and in night observations particular care should be taken not to heat it by approximation of the light. The quicker the reading is done the better.

Notice should, of course, be taken of all sudden and remarkable changes of temperature, although such occasional observations must not be recorded in the regular series.

The self-registering thermometers should be placed with the same precautions as the standard, and so fastened as to allow of one end being detached, and lifted up to allow of the indices within the tubes sliding down to the ends of the fluid columns, which they will readily do with the assistance of occasional tapping.

The self-registering thermometers are apt to get out of order by the indices becoming entangled, or from the breaking of the column of fluid. When this happens with the spirit thermometer, it may be rectified with ease by jerking the index down to the junction of the bulb and tube. The whole of the tube will at the same time become wetted with the spirit, and by setting it on end with the bulb downwards the spirit will run together into one continuous column.

When the steel index of the mercurial thermometer becomes immersed in the mercury, it must be jerked in the opposite direction, till it, with the mercury which may be above it, is projected into the little bulb at the top of the tube. If this do not succeed, heat must be applied to the mercury-bulb, and when the index is fairly lodged in the air-bulb, by carefully warming the mercury-bulb with a spirit lamp having a very small flame, the mercury must be made to expand till it rises to the very top of the tube, and projects convexly into the air-bulb. The tube must then be placed upright, and, by tapping, the detached mercury will slip down beneath the steel index, and will fairly unite with the convex projection aforesaid. Now let the bulb cool, and the mercury will sink in one united column, and leave the index free.

Besides the regular series of observations of the temperature of the air, there are other occasional observations to be made of temperature under different circumstances, which might possess great interest.

The surface temperature of the water of the sea or of rivers may be conveniently obtained by taking up a bucket-full of water and stirring round the thermometer in it.

The temperature of the water of deep wells may be ascertained in the same way, and should be taken monthly, if near the residence of the observer. The temperature of rain should also be attended to at times; it may be determined by receiving the rain in a *linen* funnel, totally enclosed in a tin case to prevent cooling by evaporation from the linen.

The temperature of the soil at different depths is a point of considerable importance. For this purpose excavations should be made in a dry sheltered situation, 3, 6, and 9 feet deep, and lined with brick or earthenware tubes. In the bottom of these excavations earthenware quart bottles may be carefully placed, filled with water, spirit, or brine, and corked. They must be carefully covered with tow or cotton, and drawn up on the 21st of every month (being the day of horary observation), and their temperatures taken by an accurate thermometer, and registered apart.

As a general caution it may be mentioned, that the *standard* thermometer should never be exposed to risk by application to such purposes, but thermometers which have been compared and corrected by comparison with it.

3. Actinometers.

Amongst the observations of highest importance must be ranked those of the force of solar and terrestrial radiation. The most perfect means of observing the former is afforded by the actinometer.

This instrument consists of a large hollow cylinder of glass, soldered at one end to a thermometer-tube, terminated at the upper end by a ball drawn out to a point, and broken off, so as to leave the end open. The other end of the cylinder is closed by a silver or silver-plated cap, cemented on it, and furnished with a screw, also of silver, passing through a collar of waxed leather, which is pressed into forcible contact with its thread, by a tightening-screw of large diameter enclosing it, and working into the silver cap, and driven home by the aid of a strong steel key or wrench, which accompanies the instrument.

The cylinder is filled with a deep blue liquid (ammonio-sulphate of copper), and the ball at the top being purposely left full of air, and the point closed with melted wax, it becomes, in any given position of the screw, a thermometer of great delicacy, capable of being read off on a divided scale attached. The cylinder is enclosed in a chamber blackened on three sides, and on the fourth, or face, defended from currents of air by a thick glass, removeable at pleasure.

The action of the screw is to diminish or increase at pleasure the capacity of the hollow of the cylinder, and thus to drive, if necessary, a portion of the liquid up into the ball, which acts as a reservoir, or, if necessary, to draw back from the reservoir such a quantity as shall just fill it, leaving no bubble of air in the cylinder.

To use the instrument, examine first whether there be any air in the cylinder, which is easily seen by holding it level, and tilting it, when the air, if any, will be seen to run along it. If there be any, hold it upright in the left hand, and the air will ascend to the root of the thermometer-tube. Then, by alternate screwing and unscrewing the screw with the right hand, as the case may require, it will always be practicable to drive the air out of the cylinder into the ball, and suck down liquid, if any, from the ball, to supply its place, till the air is entirely evacuated from the cylinder, and the latter, as well as the whole stem of the thermometer-tube, is full of the liquid in an unbroken column. Then, holding it horizontally, face upwards, slowly and cautiously unscrew the screw, till the liquid retreats to the zero of the scale.

The upper bulb is drawn out into a fine tube, which is stopped with wax. When it is needed to empty, cleanse, and refill the instrument, liquid must first be forced up into the ball, so as to compress the air in it. On warming the end, the wax will be forced out, and the screw being then totally unscrewed, and the liquid poured out, the interior of the instrument may be washed with water slightly acidulated, and the tube, ball, &c. cleansed, in the same way, after which the wax must be replaced, and the instrument refilled.

To make an observation with the actinometer, the observer must station himself in the sunshine, or in some sharply terminated shadow, so that without inconvenience, or materially altering his situation, or the exposure of the instrument in other respects, he can hold it at pleasure, either in full sun or total shadow. If placed in the sun, he must provide himself with a screen of pasteboard or tin plate, large enough to shade the whole of the lower part or chamber of the instrument, which should be placed not less than two feet from the instrument, and should be removeable in an instant of time. The best station is a room with closed doors, before an open window, or under an opening in the roof into which the sun shines freely. Draughts of air should be prevented as much as possible. If the observations be made out of doors, shelter from gusts of wind, and freedom from all penumbral shadows, as of ropes, rigging, branches, &c. should be sought. Generally, the more the observer is at his ease, with his watch and writing-table beside him, the better. He should have a watch or chronometer beating at least twice in a second, and provided with a second hand; also a pencil and paper ruled, according to the form subjoined, for registering the observations. Let him then grasp the instrument in his left hand, or if he have a proper stand (which is preferable on shore or in a building*), otherwise firmly support it, so as to expose its face perpendicularly to the direct rays of the sun, as exactly as may be.

The liquid, as soon as exposed, will mount rapidly in the stem. It should be allowed to do so for three or four minutes before the

* This may consist of two deal boards, 18 inches long, connected by a hinge, and kept at any required angle by an iron, pointed at each end. The upper should have a little rabbet or moulding fitting loosely round the actinometer, to prevent its slipping off.

observation begins, taking care, however, not to let it mount into the bulb, by a proper use of the screw. At the same time the tube should be carefully cleared (by the same action) of all small broken portions of liquid remaining in it, which should all be drawn down into the bulb. When all is ready for observation, draw the liquid down to zero of its scale, gently and steadily; place it on its stand, with its screen before it, and proceed as follows.

Having previously ascertained how many times (suppose 20) the watch beats in five seconds, let the screen be withdrawn at ten seconds before a complete minute shown by the watch, suppose at $2^h\ 14^m\ 50^s$. From 50^s to 55^s, say 0, 0, 0, ... at each beat of the watch, looking meanwhile that all is right. At 55^s complete, count 0, 1, 2, ... up to 20 beats, or to the whole minute, $2^h\ 15^m\ 0^s$, keeping the eye not on the watch, but on the end of the rising column of liquid. At the 20th beat read off, and register the reading (12°·0), as in column 3, A, of the annexed form. Then wait, watching the column

1. Date and times of observation. Feldhausen, 1837, Oct. 30.		2. Exposure, sun(☉) or shade ×.	3. Readings of the instrument.		4. Change per minute. A – B.	5. Radiation in parts of scale.	6. Remarks.
Initial.	Terminal.		A. Initial.	B. Terminal.			
h m s	m s						
2 15 0	16 0	☉	+ 12·0	+ 43·3	+ 31·3	....	The times are reduced to *apparent* time, or to the sun's hour angle from the meridian.
16 30	17 30	×	45·2	42·8	– 2·4	34·75	
18 0	19 0	☉	14·8	48·2	+ 33·4	35·40	Zero withdrawn.
19 30	20 30	×	28·0	26·8	– 1·4	34·85	
21 0	22 0	☉	9·4	43·9	+ 33·5	34·75	
22 30	23 30	×	46·6	45·5	– 1·1	34·95	
24 0	25 0	☉	9·0	43·2	+ 34·2	....	General mean per formula = 34·73 for $2^h\ 20^m\ 0^s$ of apparent time.

of air above the liquid, to see that no blebs of liquid are in it, or at the opening of the upper bulb (which will cause the movement of the ascending column to be performed by starts), till the minute is nearly elapsed. At the 50th second begin to watch the liquid rising; at 55^s begin to count 0, 1, 2, up to 20 beats, as before, attentively watching the rise of the liquid; and at the 20th beat, or complete minute ($2^h\ 16^m\ 0^s$) read off, and instantly shade the instrument, or withdraw it *just out* of the sun and penumbra. Then register the reading off (43°3) in column 3, B, and prepare for the shade observation. All this may be done without hurry in 20 seconds, with time also to withdraw the screw if the end of the column be inconveniently high in the scale, which is often required. At the 20th second prepare to observe; at the 25th begin to count beats, 0, 1, 2, ... 20; and at the 20th beat, i. e. at $2^h\ 16^m\ 30^s$, read off, and enter the reading in column 3, A, as the initial shade reading (45°·2). Then wait as before till nearly a minute has elapsed, and at $2^h\ 17^m\ 20^s$ again prepare. At $17^m\ 25^s$ begin to count beats; at $17^m\ 30^s$ read off, and enter this *terminal* shade reading (42°·8) in column 3, B, and if needed, withdraw the zero.

Again wait 20^s, in which interval there is time for the entry, &c. At $17^m\ 50^s$ remove the screen, or expose the instrument in the sun; at 55^s begin to count beats; and at the complete minute, $18^m\ 0^s$, read off ($14^\circ{\cdot}8$), and so on for several alternations, *taking care to begin and end each series with a sun observation.* If the instrument be held in the hand, care should be taken not to change the inclination of its axis to the horizon between the readings, or the compressibility of the liquid by its own weight will produce a very appreciable amount of error.

In the annexed form column 1. contains the times, initial and terminal of each sun and shade observation. Column 2. expresses by an appropriate mark, $\odot$ and $\times$, the exposure, whether in sun or shade. Column 3. contains the readings, initial and terminal (A and B). Column 4. gives the values of B — A, with its algebraical sign expressing the rise and fall per minute. And here it may be observed, that if by forgetfulness the exact minute be passed, the reading off may be made at the next 10^s, and in that case the entry in column 4 must be not the *whole* amount of B — A, but only $\frac{6}{7}$ths of that amount, so as to reduce it to an interval of 60^s precise. Column 5. contains the radiations as derived from successive triplets, $\odot \times \odot$, $\times \odot \times$, $\odot \times \odot$, &c. by the formula presently to be stated; and in column 6. are entered remarks, such as the state of the sky, wind, &c.; as also (when taken) the sun's altitude, barometer, thermometer, and other readings, &c.

The formula of reduction is as follows. Let $\odot$, $\times$, $\odot'$, $\times'$, $\odot''$, $\times''$, &c. represent the numbers in column 4, with their signs in order, as they stand, or the values of B — A. Then will the numbers in column 5 be respectively,

$$+\frac{\odot + \odot'}{2} - \times$$

$$-\frac{\times + \times'}{2} + \odot'$$

$$+\frac{\odot' + \odot''}{2} - \times'$$

$$-\frac{\times' + \times''}{2} + \odot'',$$

and so on, the algebraic signs being carefully attended to. Thus

$$34{\cdot}75 = +\frac{31{\cdot}3 + 33{\cdot}4}{2} + 2{\cdot}4$$

$$35{\cdot}40 = +\frac{2{\cdot}4 + 1{\cdot}4}{2} + 33{\cdot}4, \text{ \&c.}$$

The mean of a series not exceeding three or four triplets may be had by the formula

$$\frac{\odot + \odot' + \odot'' + \text{\&c.}}{n} - \frac{\times + \times' + \text{\&c.}}{n-1},$$

where n is the number of sun observations, the time corresponding being the middle of the middle shade observation.

A complete actinometer observation cannot consist of less than three sun and two shade observations intermediate; but the more there are taken the better, and in a very clear sunny day it is highly desirable to continue the alternate observations for a long time, even from sunrise to sunset, so as to deduce by a graphical projection the law of diurnal increase and diminution of the solar radiation, which will thus readily become apparent, provided the perfect clearness of the sky continue,—an indispensable condition in these observations, the slightest cloud or haze over the sun being at once marked by a diminution of resulting radiation.

To detect such haze or cirrus, a brown glass applied before the eye is useful, and by the help of such a glass it may here be noticed that solar halos are very frequently to be seen when the glare of light is such as to allow nothing of the sort to be perceived by the unguarded eye.

It is, as observed, essential that the instrument be exposed a few minutes to the sun, to raise its temperature in some slight degree. If this be not done, owing to some cause not very obvious, the first triplet of observations (sun, shade, sun) will give a radiation perceptibly in defect of the truth, as will become distinctly apparent on continuing the series. But it may be as well for a beginner to commence at once reading as soon as the instrument is exposed, and reject the first two triplets, by which he will see whether he has all his apparatus conveniently arranged, and get settled at his post.

When a series is long continued in a good sun, the instrument grows very hot, and the rise of the liquid in the sun observation decreases, while the fall in the shade increases; nay, towards sunset it will fall even in the sun. This phenomenon (which is at first startling, and seeming to impeach the fidelity of the instrument) is, in fact, perfectly in order, and produces absolutely no irregularity in the resulting march of the radiation. Only it is necessary in casting up the result (in col. 5.) to attend carefully to the algebraic signs of the differences in column 4, as in the following example (which, as well as that above given, is one of actual occurrence).

1. Date and times of observation. Wynberg, Nov. 24, 1837.		2. Exposure, sun or shade.	3. Readings of the Instrument.		4. Change per minute. B − A	5. Radiation in parts of scale.	6. Remarks.
Initial.	Terminal.		A. Initial.	B. Terminal.			
h m s	m s						
6 5 15	....	...	....	....	....	....	Alt. of ☉ = 7° 19′.
9 0	10 0	☉	+ 9·0	+ 9·7	+ 0·7		
10 30	11 30	×	23·0	10·8	− 12·2	11·25	
12 0	13 0	☉	34·0	31·4	− 2·6	9·25	
13 30	14 30	×	28·5	17·0	− 11·5	8·20	Cirrous haze coming on.
15 0	16 0	☉	12·0	8·0	− 4·0		
6 19 15	....	...	....	....	....	....	Alt. of ☉ = 4° 37′.

Every series of actinometer observations should be accompanied with notices in the column of remarks of the state of the wind and sky generally, the approach of any cloud (as seen in the coloured glass) near to the sun; the barometer and thermometers, *dry* and *wet*, should especially be read off more than once during the series, if a long one, and, if kept up during several hours, hourly. The times should be correct to the nearest minute, at least as serving to calculate the sun's altitude; but if this be taken (to the nearest minute or two) with a pocket sextant, or even by a style and shadow, frequently (at intervals of an hour or less) when the sun is rising or setting, it will add much to the immediate interest of the observations. When the sun is near the horizon, its reflection from the sea, or any neighbouring water, must be prevented from striking on the instrument; and similarly of snow in cold regions, or on great elevations in alpine countries.

Every actinometer should be provided with a spare glass, and all the glasses should be marked with a diamond; and it should always be noted at the head of the column of remarks; which glass is used, as the co-efficient of reduction from the parts of the scale (which are arbitrary) to parts of the *unit of radiation* varies with the glass used.

In the case of the actinometers sent out with the Expedition and to the fixed observatories, these co-efficients will be ascertained for each instrument and for each glass, *provided it be practicable to procure any observations of the sun in the interval before the sailing of the ships*; but at all events, an approximate value of the parts of the scale in *actines* will be given by measurement of the dimensions, and the glasses as well as the cylinders and capillary stems of the instruments, if accidentally broken, should have their fragments carefully preserved and labeled.

The unit of solar radiation to be adopted in the ultimate reduction of the actinometric observations is the *actine*, by which is understood that intensity of solar radiation, which at a vertical incidence, and supposing it wholly absorbed, would suffice to melt one millionth part of a metre in thickness, from the surface of a sheet of ice horizontally exposed to its action per minute of mean solar time; but it will be well to reserve the reduction of the radiations as expressed in parts of the scale to their values in terms of their unit until the final discussion of the observations.

Meanwhile, no opportunities should be lost of *comparing* together the indications of different actinometers under similar and favourable circumstances, so as to establish a correspondence of scales, which in case of accident happening to one of the instruments, will preserve its registered observations from loss.

The comparison of two actinometers may be executed by one observer using alternately each of the two instruments, thus,

Instrument A.	Instrument B.	A.	Etc.
$\odot$	$\odot$	$\odot$	
$\times$	$\times$	$\times$	
$\odot$	$\odot$	$\odot$	

beginning and ending with the same; though it would be more conveniently done by two observers observing simultaneously at the same place, and each registering his own instrument. An hour or two thus devoted to comparisons in a calm clear day, and under easy circumstances, will in all cases be extremely well bestowed.

Neither should each observer neglect to determine for himself the heat stopped by each of his glasses. This may be done also by alternating triplets of observation made with the glass on and off, thus,

Glass off.	Glass on.	Glass off.	Etc.
⊙........	⊙........	⊙........	
×........	×........	×........	
⊙........	⊙........	⊙........	

beginning and ending with the glass off, and (as in all cases) begining and ending each *triplet* with a sun observation. For the purpose now in question a very *calm* day must be chosen, and a great many triplets must be taken in succession. It will be found that a single thickness of the ordinary bluish or greenish plate glass stops about 0·20 ($=\frac{1}{5}$) of the incident calorific rays; a second glass about 0·16 (or a materially less proportion) of those which have escaped the action of the first. No two glasses, however, are precisely alike in this respect.

Very interesting observations may be made by two observers furnished with well-compared actinometers, the one stationed at the summit, the other at the foot of some great elevation, especially if the stations can be so selected that the observers shall be nearly in the line of the incident sunbeam at the time of observation, so as both to lie in the atmospheric column traversed by the rays. Many convenient stations of this kind might be found in mountainous countries; and by repeating the observation two or three times under favorable circumstances, interchanging observers and instruments, &c., and accompanying the observations with all circumstantial and local elements of precision, there is no doubt that the co-efficient of extinction of solar heat in traversing at least the lower strata of our atmosphere might be obtained with much exactness, and thus a highly valuable datum secured to science. The observers would, of course, agree to make their observations strictly simultaneous, and should, therefore, compare watches before parting.

The actinometer is also well calculated for measuring the defalcation of heat during any considerable eclipse of the sun, and the Council would point out this as an object worthy of attention, both at the fixed stations and on board the vessels; as many eclipses invisible or insignificant in one locality, are great, or even total in others. The observations should commence an hour at least before the eclipse begins, and be continued an hour beyond its termination, and the series should be uninterrupted, leaving to others to watch the phases of the eclipse. The atmospheric circumstances should be most carefully noted during the whole series.

Though out of the question in the circumstances immediately under contemplation, it may not be amiss to remind aeronauts, that observations of the actinometer may, no doubt, be made with considerable ease and precision in the car of a balloon, and if accompanied with good barometric and hygrometric simultaneous observations aloft and below, would in every point of view be most precious, thus adding one to the many useful subjects of inquiry in those hitherto almost useless adventures.

4. Radiating Thermometers.

As, however, the actinometer can only be observed at intervals in perfectly clear weather, additional information with regard to solar radiation, of much interest, though not of so precise a nature, may be obtained, by the daily register of the maximum temperature of a register thermometer, with a blackened bulb exposed to the full action of the sun's rays. It may be placed about an inch above the bare soil, and screened from currents of air. The maximum temperature indicated by such a thermometer, even in cloudy weather, will generally be considerably above that of the air, and the maxima and mean daily maxima of its indications will, after a long series of observations, afford data of the utmost value to the history of climates. The bulb of the thermometer should be about half an inch in diameter, and it may be uniformly blackened with lampblack and varnish. The graduation should be made upon the glass stem, to prevent any inconvenience from the expansion and warping of the scale.

The measure of terrestrial radiation is of no less importance to the science of meteorology than that of solar radiation, but no perfect instrument has yet been contrived for its determination. Very valuable information, however, may be derived from the daily register of the minimum temperature of a register spirit-thermometer, the bulb of which is placed in the focus of a parabolic metallic mirror, turned towards the clear aspect of the sky, and screened from currents. The mirrors furnished to the observatories are of silver-plated copper, but planished tin-plate or zinc might be substituted without detriment. They are 6 inches diameter and 2 inches deep, and the thermometers which are graduated upon the stems pass through sockets in their sides, in which they may be accurately adjusted by corks. Their bulbs do not exceed half an inch in diameter.

Even in the daytime a thermometer so placed, and turned towards the clear sky, but away from the rays of the sun, will fall several degrees below the temperature of the surrounding air.

5. Hygrometers.

Times of observation.—Observations of the *dew-point* hygrometer are as desirable at the regular hours as those of the other meteorological instruments; but, as more difficulty attends the observation, it is more liable to omission, and it is of great importance that when

one experiment only can be made, the most advantageous hour should be selected for the purpose. Now it is probable that the minimum temperature of the air in the 24 hours may correspond with the minimum temperature of the dew-point; and for the attainment of a mean result, the time of the highest dew-point should be selected, which would not differ much from 3 P.M., at which hour the observation should on no account be omitted. The hygrometer should also be observed, if possible, at 9 A.M. and 9 P.M., but the minimum temperature might probably be substituted for the 3 A.M. observation without any material error.

Occasional observations of the dew-point under peculiar circumstances, as for instance in the inhabited apartments of houses or between the decks of the ships when laid up in their winter quarters in the polar regions, could not but afford information of high practical importance.

All the ether of the dew-point hygrometer should be driven by the warmth of the hand from the covered ball into the uncovered, previously to an observation, and the ether should be dropped from a dropping-bottle very slowly upon the former. The temperature of the interior thermometer should be carefully noted upon the first appearance of the ring of dew upon the black bulb, and also its temperature upon its disappearance: the mean of the two observations, should they differ, may be entered as the dew-point, together with the temperature of the air by the exterior thermometer.

The *wet-bulb* hygrometer can be observed without difficulty, by mere inspection, and the observation should never be neglected at the regular hours. It is probable that the temperature of evaporation thus ascertained may afford the means of accurately determining the dew-point, and of solving all the points of hygrometry; but until all the necessary corrections shall have been agreed upon, one of the most essential requisites must be its frequent and accurate comparison with the dew-point, directly ascertained.

The hygrometers should be placed in the observatory, near to the standard thermometer, with which they should be frequently compared.

6. Vanes, Anemometers, and Rain Gauges.

The magnetic observatories and the Antarctic Expedition have been furnished with Osler's self-registering anemometer and rain-gauge.

In this instrument the direction of the wind is obtained by means of the vane attached to the rod, or rather tube, that carries it, and consequently causes the latter to move with itself. At the lower extremity of this tube is a small pinion working in a rack, which slides backwards and forwards, as the wind moves the vane; and to this rack a pencil is attached, which marks the direction of the wind on a paper ruled with the cardinal points, and so adjusted as to progress at the rate of 1 inch per hour, by means of a clock; the force is at the same time ascertained by a plate 1 foot square, placed at right

angles to the vane, supported by two light bars running on friction-rollers, and communicating with three spiral springs in such a way that the plate cannot be affected by the wind's pressure without instantly acting on the springs, and communicating the quantum of its action by a wire passing down the centre of the tube, to another pencil below, which thus registers its degree of force. The rain is registered at the same time by its weight acting on a balance, which moves in proportion to the quantity falling, and has also a pencil attached to it, recording the results. The receiver is so arranged as to discharge every half inch that falls, when the pencil again starts at zero.

It is probable that the results obtained with this instrument would require correction for the varying effects of the eddy, which must be formed behind the board before they can be considered as exact measures of the pressure; and the effects of variations of temperature upon the force of the springs should be experimentally ascertained, particularly in very cold climates. This latter point may be determined by measuring the compression directly by the application of known weights.

Another self-registering anemometer has recently been constructed by Professor Whewell, which exhibits upon a diagram not only the direction and force, but the direction and integral effect of the wind, but which is more complex in its construction, and practically more liable to derangement.

In it a small set of windmill vanes, something like the ventilators of windows, are presented to the wind by a common vane, in whatever direction it may blow. The current, as it passes, sets these vanes in rapid motion, and a train of wheels and pinions reduces the motion, which is thence communicated to a pencil traversing vertically, and pressing against an upright cylinder, which forms the support of the instrument: 1000 revolutions of the fly only cause the pencil to descend $\frac{1}{20}$th of an inch. The surface of the cylinder is covered with white paper, and the pencil, as the vane wavers, keeps tracing a thick irregular line, like the shadings on the coast of a map. The middle of the line may be easily traced, and it gives the mean direction of the wind, while the length of the line is proportional to the velocity of the wind and the length of time during which it blows in each direction.

Those who do not possess a register-anemometer may make use of the common vane and Lind's wind-gauge. The position of the former should be clear of all deflections and eddies from objects of the same or a higher level, and of course its position with regard to the true north should be clearly determined. In registering the direction of the wind it may be sufficient to use only 16 points of the compass.

Lind's wind-gauge for measuring the force or momentum of the wind is adjusted for use by filling it with water till the liquid in both legs of the siphon corresponds with the 0° of the scale. It is to be held perpendicularly, with the mouth of the kneed tube turned towards the wind, and the amount of the depression in one leg, and

that of its elevation in the other, are to be carefully noted. The sum of the two is the height of a column of water which the wind is capable of sustaining at the time, and every body that is opposed to that wind will be pressed upon by a force equivalent to the weight of a column of water, having its base equal to the surface that is opposed, and its height equal to the altitude of the column of water sustained by the wind in the wind-gauge.

The height of this column being given, the force of the wind on a foot square is easily found by a table which will be given in the appendix.

The observation of the gauge should always be made at the same point of a free space, and in gusty weather the maximum of the oscillation recorded. The most proper periods will be those of the other regular observations; but in great storms, or under other particular circumstances, occasional observations should be made, and registered apart.

Even in observatories which are provided with Osler's apparatus it is desirable that an accurate comparison should be made of the two anemometers.

The points most important to remark respecting the wind are,

1st. Its average intensity and general direction during the several portions of the day devoted to observation.

2ndly. The hours of the day or night when it commences to blow from a calm, or subsides into one from a breeze.

3rdly. The hours at which any remarkable changes of its direction take place.

4thly. The course which it takes in veering, and the quarter in which it ultimately settles.

5thly. The usual course of *periodical winds*, or such as remarkably prevail during certain seasons, with the law of their diurnal progress, both as to direction and intensity; at what hours, and by what degrees they commence, attain their maximum, and subside; and through what points of the compass they run in so doing.

6thly. The existence of crossing currents at different heights in the atmosphere, as indicated by the courses of the clouds in different strata.

7thly. The times of setting-in of remarkably hot or cold winds, the quarters from which they come, and their courses, as connected with the progressive changes in their temperature.

8thly. The connexion of rainy, cloudy or fair weather, with the quarter from which the wind blows, or has blown for some time previously.

The Rain-gauge may be of very simple construction. A cubical box of strong tin or zinc, exactly 10 inches by the side, open above, receives at an inch below its edge a funnel, sloping to a small hole in the centre. On one of the lateral edges of the box, close to the top of the cavity, is soldered a short pipe, in which a cork is fitted. The whole should be well painted. The water which enters this gauge is poured through the short tube into a cylindrical glass vessel, graduated to cubic inches and fifths of cubic inches. Hence

one inch depth of rain in the gauge will be measured by 100 inches of the graduated vessel, and $\frac{1}{1000}$th inch of rain may be very easily read off.

It is very much to be desired that, being of such easy construction, more than one of these gauges should be erected, or at least one placed with its edge nearly level with the ground, and another upon the top of the highest building, rock, or tree in the immediate vicinity of the place of observation, the height of which must be carefully determined; it having been satisfactorily ascertained that the height of the gauge above the ground is a very material element in the quantity of rain which enters it. The quantity of water should be daily measured and registered at 9 A.M.

7. Clouds and Meteors.

Many very highly-interesting observations may be made, without the aid of instruments, upon the clouds. In describing them Mr. Howard's nomenclature may be adopted with great advantage. By means of the clouds different simultaneous currents of wind may often be detected, the different directions of which should be carefully ascertained by referring their motions to some fixed object. Their gradual evaporation or precipitation should also be carefully noted, and particularly their regular disappearance at night, or their more irregular and sudden formation.

Rainbows, parhelia, haloes, &c., will of course be noted amongst the occasional remarks of the register; and an attempt should be made to express approximatively by numbers, the proportion which the overcast portion of the sky may bear to the clear space. For this the hemisphere may be supposed to be divided into eight sections, and the cloudy portion may be expressed by the fraction $\frac{1}{8}$th or $\frac{5}{8}$ths, &c.

8. Electrometers.

The Council are fully impressed with the high importance of regular observations on the electrical state of the atmosphere; but they are not prepared to suggest any means of effecting this desirable object, which will at all correspond with the present advanced state of electrical physics. At no distant period they hope to supply a defect which is certainly a reproach to science. In the meantime much valuable information might be acquired by observations of an electroscope, on one of the ordinary constructions connected with a lofty insulated wire.

In erecting such a wire, proper precautions should be taken against accidents by preparing a sufficient conductor in its immediate vicinity, by which a communication could be at once opened with the ground in case of any sudden and dangerous accumulation of the electric fluid.

As a temporary contrivance, a common jointed fishing-rod, having a glass stick well varnished with shell lac, substituted for its smallest joint, may be projected into the atmosphere. To the end of the

glass must be fixed a metallic wire terminating in a point, and connected with an electroscope by means of a fine copper wire. If the wire be made to terminate in a spiral wrapped round a piece of cotton dipped in spirits of wine and inflamed, its power of collecting electricity will be sometimes doubled, but great precautions are necessary when this mode is employed. When the electroscope has been charged, the nature of the electricity may be tested in the usual way by excited glass or sealing wax.

The principal electroscopes which are capable of being employed to ascertain the electrical state of the atmosphere, or rather to compare its state at any given elevation with the state of the medium in contact with the instrument, are the following.

1. De Saussure's electrometer, which consists of two fine wires, each terminated by a small pith ball, and adapted to a small metal rod fixed in the upper part of a square glass cover, upon one of the faces of which a divided scale is marked, in order to measure the angles of deviation of the two balls.

2. Volta's electrometer, formed of two straws about 2 inches long and $\frac{1}{4}$th of a line broad, suspended from two small very moveable rings adapted to a metal rod: to measure the deviation of the straws a telescope with a nonius is employed.

3. Singer's electrometer, consisting of two slips of gold leaf suspended from the rod.

4. Bohnenberger's electroscope, formed of a single strip of gold leaf suspended from the conducting rod between two dry piles, the negative pole of one and the positive pole of the other being uppermost: this arrangement has the advantage of indicating the kind of electricity communicated to the conductor.

The observations made with these and similar instruments have demonstrated that in serene weather the electricity of the atmosphere is always positive with regard to that of the earth, and that it becomes more and more positive in proportion to its elevation above the earth's surface; so that if an observer be on a mountain or in a balloon, if his conductor be directed downwards to reach an inferior stratum of air, his electroscope will indicate negative electricity; and if it be sent upwards into a superior stratum, positive electricity will be manifested. Various means have been resorted to in these experiments, such as connecting one of the extremities of the conducting wire to a kite, a small balloon, or the head of an arrow, the other extremity remaining attached to the electroscope.

It has been ascertained by the observations of De Saussure, Schubler, Arago and others, that the positive electricity of the atmosphere is subject to diurnal variations of intensity, there being two maxima and two minima during the twenty-four hours. The first minimum takes place a little before the rising of the sun; as it rises, the intensity, at first gradually and then rapidly, increases, and arrives at its first maximum a few hours after. This excess diminishes at first rapidly and afterwards slowly, and arrives at its minimum some hours before sunset; it re-ascends when the sun approaches the horizon, and attains its second maximum a few hours after, then diminishes

till sunrise, and proceeds in the order already indicated. The intensity of the free electricity of the atmosphere has also been found to undergo annual changes, increasing from the month of July to the month of November inclusive, so that the greatest intensity occurs in winter, and the least in summer.

In cloudy weather the free electricity of the atmosphere is still positive. During storms, or when it rains or snows, the electricity is sometimes positive and sometimes negative, and its intensity is always much more considerable than in serene weather. The electroscope will, during the continuance of a storm, frequently indicate several changes, from positive to negative.

The above is a short summary of almost all that is known respecting the laws of atmospheric electricity. It will be highly important to obtain a series of observations equal in accuracy to those made by Schubler at Frankfort in 1811 and 1812, simultaneously with the observations of the hygrometer, barometer, thermometer, &c. Combined observations at a number of different stations cannot fail to give us important information respecting the distribution of the free electricity in the atmosphere, and the extent and nature of the disturbances to which it is subject; but to render the results valuable it will be necessary to have instruments comparable with each other, and this may be a difficult matter to effect*.

Very recently a new method of investigating the electric state of the atmosphere has been proposed, likely to lead hereafter to very certain and valuable results; but it has not been sufficiently put in practice to enable the Council to recommend, at the present moment, the best form of instrument for making simultaneous and comparable observations, or the proper precautions to guide the observer in manipulating it.

For the principle of this instrument we are indebted to Mr. Colladon of Geneva. He found, that if the two ends of the wire of a galvanic multiplier, consisting of very numerous coils well insulated from each other, were brought in contact, one with a body positively, and the other with a body negatively charged, a current of electricity passes through the wire, until equilibrium is restored; the energy and direction of this current is indicated by the deviation of the needle from the zero-point of the scale. This instrument is applied to the purpose of ascertaining and measuring the atmospheric electricity, by communicating one end of the wire with the earth, and allowing the other to extend into the region of the atmosphere, the electrical state of which is intended to be compared.

Thunder storms, of course, should be attended to; but it is of consequence also to notice distant lightning not accompanied with thunder audible at the place of observation, especially if it take place many days in succession, and to note the quarter of the horizon where it appears, and the extent which it embraces. In an actual thunder storm, especial notice should be taken of the quantity of rain

* For a fuller account of what is known respecting atmospheric electricity, and the mode of conducting the observations, see Becquerel's *Traité de l'Electricité*, t. iv. pp. 78—125.

which falls, and of the fits or intermittences of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind, and the apparent progress of the storm with or against it*.

9. Registers.

The Register proposed by the Council may be comprised in two skeleton forms, which have been supplied to the magnetical observatories and to the Expedition.

They are each calculated for one month's observation. *The first form* is for the insertion of observations as they are made in their uncorrected state. It consists of 12 principal divisions, and is ruled across for 31 days, and for the arithmetical convenience of casting up the sums and means of the quantities inserted. At the bottom of the sheet there is also a space provided for the hourly observations of the barometer and thermometers on *the twenty-first day of the month*, which will be more particularly described after the explanation of the principal divisions.

The outside compartments, both on the left and right of the sheet, are for the date of the month and the phases of the moon.

The second compartment is for the height of the barometer, and the temperature of the mercury for the four regular periods of observation.

The third compartment is appropriated to the dew-point hygrometer, and contains also four columns for the four daily observations, each of which is subdivided into three; for the temperature of the air, the dew-point, and the difference between the two.

The fourth compartment is for the wet-bulb hygrometer, and is similarly divided and subdivided for the temperature of the dry- and wet-bulb thermometer, and for their differences.

The fifth compartment is prepared for the maxima and minima of temperature, and is divided into three. In the first division are to be recorded the maxima and minima of thermometers carefully placed in the shade and screened from radiation. In the second, the maxima of a blackened thermometer exposed to the sun, and the minima of a thermometer placed in a metallic mirror, and radiating freely to the clear sky. The third is devoted to occasional observations of the actinometer under favourable circumstances.

The sixth compartment is for the temperature of the surface-water of the sea, or of any river in the immediate neighbourhood of the observatory.

The seventh compartment is prepared for observations upon the direction and force of the wind at the four regular hours of registry. In the left-hand column of each division is to be recorded the direction of the vane, and in the right-hand column the height of Lind's gauge, in tenths of an inch of water.

In the eighth compartment the amount of rain is to be registered once in the day; and in the ninth, the electrical state of the atmo-

* On these subjects the Council especially recommend the attentive perusal of Arago's *Notice sur le Tonnerre.*

sphere, if possible, at the four periods, 3 A.M., 9 A.M., 3 P.M., and 9 P.M.

The tenth compartment is appropriated to remarks on the clouds, and weather generally; and in the eleventh is to be noted, at noon, the longitude and latitude at sea.

On a careful review of the month's observations, the maxima and minima results should have the algebraic signs + and — respectively affixed.

The second form is devoted to the corrected results of the observations, and to the optical comparison together of some of them, by their projection upon a scale of equal parts.

The upper half of the sheet is vertically divided into two equal parts, each prepared for half the month's observations, and accordingly ruled across into sixteen spaces for the daily observations, and two for the sums and means of the quantities. Each half is also divided into five compartments.

The first is for the date of the month and the phases of the moon.

The second for the corrected height of the barometer at 32° Fahr.

The third is appropriated to the elastic force of the aqueous vapour corresponding to the dew-point, and which may be taken from Table 5. in the Appendix B.

The fourth is for the maximum and minimum of temperature, and the mean of the two.

And the fifth for occasional remarks.

The lower half of the sheet is also vertically divided into two equal parts, each of which is similarly divided into 31 columns for the daily observations of a month; and these again subdivided into four, for the six-hourly observations of each day. The vertical lines thus formed are divided into 6 inches; and each inch into tenths of an inch, and half-tenths, by horizontal lines.

The left-hand compartment thus ruled, is intended for the projection of curves of temperature; for this purpose each tenth of an inch upon the scale must be reckoned a degree, which will be divided by the faint line into halves.

The value of the degree may be arbitrarily fixed, and inserted in the margin according to convenience. Towards the upper part of the scale the results of the six-hourly observations should each be marked by a dot in its appropriate space, and the dots may be afterwards connected by a line.

The temperatures of the dew-point, or of the wet-bulb thermometer, or the mean temperature, may be compared with this primary result by projecting their curves in a similar way beneath it; and should the observations of these points be less frequent than four times in the day, the daily spaces may easily be divided accordingly.

The right-hand compartment is appropriated to the projection of curves of pressure, and the four daily observations of the barometer are to be marked by dots towards the upper part of the scale of inches, and afterwards connected by a line. Towards the lower part of the scale the elastic force of the vapour is to be noted, and the marks to be similarly connected by a line.

On either the scale of temperature or of pressure, occasional comparisons may be made with results obtained at other stations, which, if judiciously selected, cannot fail to prove of high interest and importance. They should, however, be laid down in pencil, or marked by a fainter line.

At the bottom of the first skeleton form will be found a space prepared for the 24 hourly observations of the *twenty-first day* of the month, both in their uncorrected and their corrected state. It is divided into four compartments for 6 hours each. The instruments which can with most facility be observed in this manner, are the barometer with its attached thermometer, and the dry- and wet-bulb thermometers; and columns are appropriated to each of these. It is desirable that the means of each 6 hours should be calculated, and spaces have been provided accordingly for the arithmetical operations.

In casting up the sums and calculating the *means*, care should be taken in all cases to verify the results by repetition; and the Council recommend in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error) may remain to vitiate the mean result. The precaution should also be taken of counting the days in each column, so as to make no mistake in the divisor.

The skeleton forms will be interleaved with blank pages, to facilitate computations and comparisons, and to afford space for other observations of atmospheric phenomena, which will perpetually present themselves to those who make it their business or their pleasure to watch the changes of the weather on a judicious plan. The Council, indeed, wish it to be understood, that, in the suggestions which they have offered, they have taken into consideration only such observations as are indispensable for laying the first foundations of meteorological science; some investigations of a more refined character they may, probably, make the subject of a future report.

As soon as the register of a month's observations has been computed, it should be copied, and the copy carefully compared with the original by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts,—a process always advisable whenever great masses of figures are required to be correctly copied.

A copy so verified should be transmitted regularly to such person or public body, as, under the circumstances, may be authorized or best adapted to receive and discuss the observations.

ACCOUNT OF THE MAGNETICAL INSTRUMENTS EMPLOYED, AND OF THE MODE OF OBSERVATION TO BE ADOPTED, IN THE MAGNETICAL OBSERVATORIES ABOUT TO BE ESTABLISHED BY HER MAJESTY'S GOVERNMENT.

The elements on which the determination of the earth's magnetic force is usually based are, the *declination*, the *inclination*, and the *intensity*. If a vertical plane be conceived to pass through the direction of the force, that direction will be determined when its inclination to the horizon is given, as well as the angle which the plane itself forms with the meridian; and if, in addition to these quantities, we likewise know the number which expresses the ratio of the intensity of the force to some established unit, it is manifest that the force is completely determined.

For many purposes, however, and especially in the delicate researches connected with the *variations* of the magnetic force, a different system of elements is preferable. The intensity being resolved into two portions in the plane of the magnetic meridian, one of them *horizontal* and the other *vertical*, it is manifest that these two components may be substituted for the total intensity and the inclination; while, at the same time, their changes may be determined with far greater precision. The former variables are connected with the latter by the relations

$$X = R\cos\theta, \qquad Y = R\sin\theta;$$

in which R denotes the intensity, X and Y its horizontal and vertical components, and θ the inclination; and the variations of θ and R are expressed in terms of the variations of X and Y by the formulæ:

$$d\theta = \tfrac{1}{2}\sin 2\theta\left(\frac{dY}{Y} - \frac{dX}{X}\right);$$

$$\frac{dR}{R} = \cos^2\theta\frac{dX}{X} + \sin^2\theta\frac{dY}{Y}.$$

As the instruments destined for the observation of these elements (with a set of which each observatory is furnished) are, for the most part, novel in form, it will be useful to give a somewhat detailed account of their construction and various adjustments, before entering on the plan of observation to be pursued.

Declination Magnetometer.

Construction.—The essential part of the declination magnetometer is a magnet bar, suspended by fibres of untwisted silk, and inclosed in a box, to protect it from the agitation of the air. The bar is a rectangular parallelopiped, 15 inches in length, $\frac{7}{8}$ths of an inch in breadth, and $\frac{1}{4}$th of an inch in thickness. In addition to the stirrup by which the bar is suspended, it is furnished with two sliding pieces,

one near each end. One of these pieces contains an achromatic lens, and the other a finely divided scale of glass; the scale being adjusted to the focus of the lens, it is manifest that the apparatus forms a moving collimator, and that its absolute position at any instant, as well as its changes of position from one instant to another, may be read off by a telescope at a distance. The aperture of the lens of this collimator is $1\frac{1}{4}$ inch, and its focal length about 12 inches. Each division of the scale is $\frac{1}{383}$th part of an inch; and the corresponding angular quantity is about 43 seconds.

To the suspension thread is attached a small cylindrical bar, the ends of which are of smaller diameter, and support the stirrup which carries the magnet. The apertures in the stirrup, by which it hangs on the cylinder, are of the form of inverted Y s, so that the bearing points are invariable. A second pair of apertures at the other side of the magnet, serves for the purpose of *inversal*; and care has been taken to render the lines connecting the bearing points of each pair of Y s parallel, so that there may be no difference in the amount of torsion of the thread in the two positions of the stirrup. The two pairs of apertures are at different distances from the magnet, in order that the line of collimation may remain nearly at the same height on inversal, and thus it may not be necessary to alter the length of the suspension thread. The stirrup, and the other sliding pieces, are formed of gun metal.

For the purpose of taking out the torsion of the suspension thread, the apparatus is furnished with a *detorsion bar*, which (with its appendages) is of the same weight as the magnet. It is a rectangular bar of gun-metal, furnished with a stirrup and collimator similar to those of the magnet. A rectangular aperture in the middle receives a small magnet, the use of which is to impart a slight directive force to the suspended bar, and without which the final adjustment of detorsion would be tedious and difficult.

The frame-work of the instrument consists of two pillars of copper, 35 inches in height, firmly screwed to a massive marble base. These pillars are connected by two cross pieces of wood, one at the top, and the other 7 inches from the bottom. In the centre of the top piece is the suspension apparatus, and a divided circle used in determining the amount of torsion of the thread. A glass tube (between this and the middle of the lower cross piece) incloses the suspension thread; and a glass cap at top covers the suspension apparatus, and completes the inclosure of the instrument.

The box is cylindrical, its dimensions being 20 inches in diameter by 7 inches in depth. It rests upon the marble slab, and encompasses the pillars; and it is so contrived as to be raised, when necessary, for the purpose of manipulation. There are two apertures in the box, opposite to each other. The aperture in front, used for reading, is covered with a circular piece of parallel glass, attached to a rectangular frame of wood which moves in dovetails; the prismatic error of the glass (if any) is corrected by simply reversing the slider in the dovetails. The opposite aperture is for the illumination of the scale.

In addition to the parts abovementioned, the instrument is provided with a second magnet, of the same dimensions as the first, to be used in measurements of absolute intensity; a thermometer, the bulb of which enters the box, in order to determine the interior temperature; and a copper ring, for the purpose of checking the vibrations.

Adjustment.—The instrument having been placed on its support, the base is to be levelled, and the whole then fixed in its place. The levelling of the base may conveniently be performed by the aid of a plumb-line hanging in the place of the suspension thread; but no great precision is required in this operation, the chief object of which is that the suspension thread may occupy the middle of the tube, and that the magnet may be central with regard to its support. The suspension thread is then to be formed, and attached at one extremity to the roller of the suspension apparatus, and at the other to the small cylinder which is to bear the stirrup and magnet. Sixteen fibres* of untwisted silk are sufficient to bear double the load without breaking, and will be found to form in other respects a convenient suspension.

These preparations being made, the adjustments are the following:

1. The sliders being placed on the magnet, the scale is to be adjusted to the focus of the lens, and in such a manner that the centre of gravity of the sliders may be near the middle of the bar. The adjustment to focus has been already made by the artist, and the corresponding distances of the sliders measured; they will be found in Table I.

2. The magnet is to be connected with the suspension thread by means of the stirrup, and to be moved in the stirrup until it assumes the horizontal position. This adjustment may be conveniently effected by means of the image of the magnet, reflected from the surface of water or mercury, the object and its reflected image being parallel when the former is horizontal. The stirrup is then fastened by its screws, and the magnet wound up to the desired height. As the thread stretches considerably at first, allowance should be made for this in the height.

3.† The magnet is then removed, and the unmagnetic bar (having its collimator similarly adjusted) is to be attached, without its small magnet, and allowed to swing for several hours. The bar having come to rest, or nearly so, its deviation from the magnetic meridian is to be *estimated*, and the moveable arm of the torsion circle turned through the same angle in an opposite direction. The plane of detorsion then coincides, approximately, with the magnetic meridian.

4. The magnet is then to be substituted for the unmagnetic bar, and the telescope being directed towards the collimator, the point of the scale coinciding with the vertical wire is to be noted when

* Not the individual fibre of the silk-worm, but the compound fibre in the state in which it is prepared for spinning.

† It is obvious that this step of the adjustment may precede the 1st and 2nd, where a saving of time is important.

the magnet is in the *direct* and *inverted* positions. Half the sum of these readings is the point of the scale corresponding to the magnetic axis of the magnet bar; and half their difference (converted into angular measure) is the deviation of the line of collimation of the telescope from the magnetic meridian. The telescope should be moved through this angle in the opposite direction.

5. In order to take out the remaining torsion of the thread, the magnet is again to be removed, and the unmagnetic bar (with its small magnet attached) substituted. The deviation of this bar from the magnetic meridian should then be read off on its divided scale, and the moveable arm of the torsion circle turned through a given angle in the opposite direction. The deviation being again read, a simple proportion will give the remaining angle of torsion; and the moveable arm being turned through this angle in the opposite direction, another observation will serve to verify the adjustment. The plane of detorsion then coincides with the magnetic meridian; and the magnet being replaced, the instrument is ready for use.

Observations.—The observations to be made with this instrument are, 1. of the *absolute declination*; 2. of the *variations of the declination*; and 3. of the *absolute intensity*.

For measurements of the *absolute declination* each observatory is furnished with a small transit instrument having an azimuth circle. This instrument being placed in the magnetic meridian of the declination instrument, the point of the scale coinciding with the central wire of the transit telescope is to be observed; the interval between this point and the point* corresponding to the magnetic axis of the bar, converted into angular measure, is the deviation (δ) of the line of collimation of the transit telescope from the magnetic meridian. The verniers of the horizontal circle being then read, the telescope is turned, and its central wire made to bisect a distant mark, whose azimuth (α) has been accurately determined. If a denote the angle read off on the horizontal circle, it is manifest that the angle between the magnetic and the astronomical meridians is

$$a + \alpha + \delta,$$

α and δ being affected with their proper signs. The angle α is supposed to have been previously determined by the help of the transit instrument.

But instead of referring the transit telescope *directly* to the magnetic meridian by means of the moving collimator, the same result will be obtained, and probably in a better manner, by referring it to the line of collimation of the *fixed telescope*, with which the changes

* In determining this point by the mean of two readings of the scale with the bar erect and inverted, care must be taken to eliminate the declination changes which may occur in the interval of the two parts of the observation. The horizontal force magnetometer may be applied to the purpose of this elimination. But perhaps the simplest course is to take a *series* of readings as rapidly as possible, alternately in the two positions of the bar, choosing for the time of observation a period when the declination changes are slow and regular. By comparing each result with the mean of the preceding and subsequent, and then taking the mean of all these partial means, a very accurate determination may be obtained.

of the declination are regularly observed. For this purpose it is only necessary to employ the latter telescope as a collimator, the telescope being *reversed* in its Y supports, if necessary. A fixed collimator may also be conveniently substituted for the distant mark. This mode of observation has the advantage of connecting the absolute determination directly with the regular series of observations; and it is manifest that it is sufficient, without any other means, to determine whether any, and what changes may have occurred in the position of the fixed telescope.

The fixed telescopes, furnished to each observatory, have an aperture of $1\frac{5}{8}$ inches, and focal length of 14 inches. They should be fixed upon a stone pillar, or upon a firm pedestal of wood resting on solid masonry unconnected with the floor.

In observing the *declination changes* the fixed telescope (above referred to) is alone employed. The observation consists simply in noting the point of the scale coinciding with the vertical wire, at three successive limits of the arc of vibration. The three readings being denoted by a, b, c, the mean point of the scale corresponding to the time of the middle observation is

$$\tfrac{1}{4}(a + 2b + c).$$

This mode of observation is sufficient where the observer is not limited to a *precise moment* of observation. Otherwise the more exact method pointed out by Gauss is to be preferred*.

The changes of position of the scale may be converted into angular measure, the angle corresponding to one division being known. In general, however, this reduction will only be required in the monthly mean results.

Before the true changes of the declination can be deduced from the observed readings, it is necessary to apply a correction depending upon the force of torsion of the suspension thread. For supposing that the plane of detorsion has been brought (by the adjustments above described) to coincide with the magnetic meridian, it is manifest that on every deviation of the magnet from that its mean position, the torsion force will be brought into play; and as this force tends to bring back the magnet to the mean position, the apparent deviations must be less than the true. The ratio of the torsion force to the magnetic directive force is experimentally determined by turning the moveable arm of the torsion circle through any given large angle (for example 90°), and observing the corresponding angle through which the magnet is deflected. Let u denote the latter angle, and v the former; then the ratio in question is

$$\frac{G}{F} = \frac{u}{v-u};$$

in which G is the coefficient of the torsion force, and F the moment arising from the action of the earth's magnetic force upon the free magnetism of the bar, the direction of the action being supposed to be perpendicular to its magnetic axis. The ratio of the two forces

* See Taylor's Scientific Memoirs, vol. ii. part v. p. 44. *et seq.*

being thus found, the true declination changes are deduced from the apparent, by multiplying them by the coefficient

$$1 + \frac{G}{F}.$$

In order to obtain an exact result by the mode of experiment above described, it is necessary that the *actual* changes of the declination which may occur in the interval of the two readings, should be eliminated. The obvious method of accomplishing this, is to observe the declination changes *simultaneously* with a second apparatus. If such means, however, should not be at hand, the object may be attained by making a *series* of readings with the vernier of the torsion circle alternately in two fixed positions (for example $+ 90°$ and $- 90°$); the mean result will be independent of the declination changes, provided the progress of these changes has been gradual in the interval of the experiment.

For the purpose of determining the *absolute intensity* of the horizontal component of the earth's magnetic force, the declination instrument is provided with a *deflecting bar*, and a *beam compass* to be used in measuring its distance from the suspended magnet. The mode of observation has been so fully explained by Gauss, in his valuable memoir entitled "*Intensitas vis terrestris ad mensuram absolutam revocata*," and in the first volume of the "*Resultate**," that it is unnecessary to enter here into any details.

The following table contains the interval of the sliders of the collimators, corresponding to focal adjustment; and also the arc values of one division of the scale in each instrument, expressed in decimals of a minute.

TABLE I.

No. of Instrument.	Observatory.	Interval of Sliders.	Arc value of one division.
		inches.	′
I.	H.M.S. Erebus	11·70	0·7267
II.	Van Diemen's Land	12·01	0·7085
III.	Montreal..........	11·72	0·7208
IV.	Cape of Good Hope	11·18	0·7525
V.	St. Helena	11·96	0·7108

HORIZONTAL FORCE MAGNETOMETER.

The instrument employed in determining the horizontal component of the earth's magnetic force is similar, in principle, to the "*bifilar magnetometer*" of Gauss. It is a magnet bar, suspended by two equidistant wires, or (more accurately) by two portions of the same wire, the distance of whose bearing points is the same above and below; by the rotation of the upper extremities of the wire

* Translated in Taylor's Scientific Memoirs, vol. ii. part v.

round their middle point, the magnet is maintained in a position at right angles to the magnetic meridian.

It is manifest from the nature of this suspension, that the *weight* of the suspended body will tend to bring it into the position in which the two portions of the wire are in the *same plane* throughout. The moment of the directive force is G sin v;—v denoting the angle formed by the lines joining the bearing points above and below, or the deviation from the plane of detorsion; and G being expressed by the formula

$$G = w \frac{a^2}{l};$$

in which w denotes the weight of the suspended body, a half the interval of the wires, and l their length. The earth's *magnetic force*, on the other hand, tends to bring the magnetic axis of the bar into the magnetic meridian, with the force F sin u; in which u is the deviation of the magnetic axis from the meridian, and F is the product of the horizontal part of the earth's magnetic force into the moment of free magnetism of the bar. The magnet being thus acted on by two forces, will rest in the position in which their moments are equal. When the instrument is so adjusted that $u = 90°$, or the magnet at right angles to the magnetic meridian,

$$F = G \sin v;$$

and the ratio of the forces is known, when we know the angle v. But as one of these forces is constant, and the other variable, it is evident that the place of the magnet will vary around its mean position, and that the variations of angle are connected with the variations of the force. This connexion is expressed by the formula

$$dF = F \cot v \,.\, du;$$

the angle $d\ u$ being expressed in parts of radius.

Construction.—The magnet bar is of the same dimensions as that of the declination instrument. The collimator, by which its changes of position are observed, is attached to the stirrup, and has a motion in azimuth. The suspending wire passes round a small grooved wheel, on the axis of which the stirrup rests by inverted Y s; and the instrument is furnished with a series of such wheels, whose diameters increase in arithmetical progression, (the common difference being about $\frac{1}{20}$th of an inch,) for the purpose of varying the interval of the wires. The exact intervals, corresponding to each separate wheel, have been determined by the artist by accurate micrometrical measurements; they are given in Table III. The same interval is altered, at the upper extremity, by means of two screws (one right-handed and the other left-handed) cut in the same cylinder; the wires being lodged in the intervals of the threads, and their distance regulated by a micrometer head. The interval of the threads of this screw (which is precisely the same for all the instruments) is $\frac{2}{77}$ths, or ·02597 of an inch. The micrometer head is divided into 100 parts; and, as one revolution of the head corresponds to *two* threads of the screw, a single division is equivalent to ·0005194, or the $\frac{1}{2000}$th of

an inch nearly. The micrometer head has been carefully adjusted by the artist, so that the index is at zero, when the interval of the wires is exactly half an inch.

The collimator, in this instrument, is inclosed in a light tube attached to the stirrup. The aperture of the lens is about $\frac{8}{10}$ths of an inch, and its focal length about 8 inches. The divisions of the scale are the same as in the collimator of the declination magnetometer; the corresponding arc values have been ascertained for each instrument by accurate experiment, and are given in Table II.

The larger parts of this apparatus,—the box, the framework, and the support,—are precisely similar to those of the declination magnetometer. In addition to the parts already described, the instrument is furnished with a spare magnet; a brass weight, required in determining the plane of detorsion of the wires relatively to the magnetic meridian; a thermometer, the bulb of which is within the box, for the purpose of ascertaining the interior temperature; and a copper ring used in checking the vibrations.

Adjustments.—The instrument being placed on its support, the base is to be levelled, and the whole apparatus fixed. Having then selected one of the small grooved wheels, and fixed it, temporarily, with its axis horizontal, the wire is to be passed round it; and the free extremities of the wire being passed through the corresponding holes in the suspension roller, placed beneath, weights are to be attached, and the two portions of the wire allowed to assume their natural position; the extremities may then be *fastened* to the roller, by introducing small wooden plugs in the holes. The parts are then to be inverted, and put in their proper places; the suspension apparatus resting on the divided circle, and the wire hanging down the tube.

The collimator (its scale having been previously adjusted to focus*) is to be screwed on to the stirrup, and the latter attached to the axis of the grooved wheel by means of its Y s. The magnet is then introduced into the stirrup, and levelled; and the wires wound upon the roller, until the collimator is at the desired height.

These preparations being made, the adjustments are the following:

1. Determine experimentally the angle through which it is necessary to turn the moveable arm of the torsion circle, in order to deflect the magnet from the magnetic meridian to a position at right angles to it, the two positions being merely *estimated*. The cosine of this angle is, approximately, the ratio of the magnetic force to the torsion force, or the value of the fraction $\frac{F}{G}$. The nearer this ratio is to unity, the more delicate will be the instrument; practically, $\frac{9}{10}$ will be found a convenient value. If, on making the foregoing experiment, the ratio should be found to fall below, or to exceed the proper limits, the torsion force must be altered by introducing a different wheel, and making the corresponding alteration in the interval of the upper extremities of the wires.

2. The magnetic axis being brought, approximately, into the mag-

* This adjustment has been already made by the artist.

netic meridian, by turning the moveable arm of the torsion circle, the collimator is to be turned, by its independent motion, until some point about the middle of the scale coincides with the vertical wire of the fixed telescope. This point of the scale is to be noted in the usual manner.

3. The magnet is then to be removed, and the brass weight attached. Note the new point of the scale which coincides with the wire of the telescope. Then, if the magnet had been placed (in the previous experiment) in its *direct* position (i. e. north to north) the error of the plane of detorsion is

$$v\left(\frac{G}{F}+1\right).$$

v being the difference of the two readings, converted into angular measure. If, on the other hand, the magnet had been *reversed* (i. e. north end to south) the error is

$$v'\left(\frac{G}{F}-1\right).$$

The moveable arm of the torsion circle is then to be turned through this angle, in the opposite direction; and the magnetic axis will be in the magnetic meridian.

The difference of the two readings, corresponding to a given error, being much greater in the reversed than in the direct position of the magnet, it follows that the former affords a much more delicate method of making the desired adjustment.

4. The brass weight remaining attached, turn the moveable arm of the torsion circle through 90°. Then turn back the collimator, until some point about the middle of the scale coincides with the vertical wire of the fixed telescope; and note the reading.

5. Now remove the brass weight, and replace the magnet. The magnetic force of the earth will bring it back towards the magnetic meridian, and the scale will be thrown out of the field of the telescope. Then turn the moveable arm of the torsion circle, until the point of the scale last noted is brought to coincide again with the wire of the telescope; the magnetic axis is then in the plane perpendicular to the magnetic meridian, and the adjustment is complete.

Observations.—The observations to be made with this instrument are those of the *absolute* value of the *horizontal intensity*, and its *changes*.

From the explanation of the principle of the instrument, given above, it is manifest that it will serve to determine the moment of the force exerted by the earth upon the free magnetism of the suspended bar. Let X denote (as before) the horizontal part of the earth's magnetic force; m the moment of free magnetism of the bar; then

$$m\,X = F,$$

F having the same meaning as before (page 230). Hence, substituting the values of F and G, we have

$$m\,X = w\,\frac{a^2}{l}\sin v\,;$$

in which equation all the quantities of the second member may be obtained by direct measurement. The chief difficulty in this method consists in the determination of the quantity a, which should be known to a very small fractional part of its actual value. This difficulty has been overcome by the measuring apparatus connected with the suspension, which (as has been already stated) serves to determine the interval of the wires, at their upper extremity, to the $\frac{1}{2000}$th of an inch. The numbers given in Table III. for the lower interval, may be relied on to the same degree of accuracy. It is scarcely necessary to mention that the length of the wires, l, is to be measured between the points of contact above and below.

The *product* of the earth's magnetic force into the magnetic moment of the bar being thus known, the *ratio* of the same quantities is to be determined by removing the bar from its stirrup, and using it to *deflect* the suspended bar of the declination instrument, according to the known method devised by Gauss. The experiments of deflection may, however, be performed without the aid of the second magnetometer, by operating upon another bar placed in the *reverse* position. This method has even the advantage in point of delicacy; but it labours under the disadvantage of requiring that the value of $\frac{\mathrm{F}}{\mathrm{G}}$ should be determined for the second bar.

The chief use of this apparatus is in observing the *variations* of the intensity. In these observations it is only necessary to note, at any moment, the point of the scale coinciding with the vertical wire of the fixed telescope, the mode of observing being precisely the same as in the other instrument. Let n be the number of divisions, and parts of a division, by which the reading at any moment differs from its mean value; then the corresponding variation of the angle (in parts of radius) is

$$d u = n a;$$

a denoting the arc value (in parts of radius) corresponding to a single division. Substituting this in the formula of page 230, we have

$$\frac{d \mathrm{F}}{\mathrm{F}} = n a \operatorname{cotan} v = k n;$$

k being the value of the constant coefficient a cotan v. The values of a have been determined for each of the instruments, and are given in Table II.

The quantity F, in the preceding formula, is the product of the earth's magnetic force into the moment of free magnetism of the bar; and, as the latter quantity varies with the temperature, it is necessary to apply a correction, before we can infer the true changes of the earth's force. This correction is easily deduced. Since $\mathrm{F} = \mathrm{X} m$, there is

$$\frac{d \mathrm{F}}{\mathrm{F}} = \frac{d \mathrm{X}}{\mathrm{X}} + \frac{d m}{m};$$

so that the correction to be applied, in order to deduce the value of $\frac{d \mathrm{X}}{\mathrm{X}}$, is $- \frac{d m}{m}$. Let t denote the temperature, in degrees of Fahren-

heit; q the relative change of the magnetic moment corresponding to one degree; then

$$-\frac{dm}{m} = q\,(t - 32).$$

Accordingly, the changes of the earth's force will be expressed by the formula

$$\frac{d\,X}{X} = k\,n + q\,(t - 32).$$

It is not necessary that these reductions should be applied to the individual results, except in cases of marked change, where it is desired to trace the progress of the actual phenomena. The results should be recorded as they are observed, in parts of the scale; and the reductions made in the monthly, or other mean values.

Table II. contains the arc values of one division of the scale, in each instrument, expressed in *decimals of a minute*; as also the same quantities reduced to *radius*, as the unit by multiplying by the number ·0002909.

Table III. contains the intervals of the axes of the wires corresponding to each wheel, in decimals of an inch; the wire used being that designated in commerce as "silver, fine 6."

TABLE II.

No. of Instrument.	Observatory.	Arc values of one division.	
		In Minutes.	In parts of Radius.
I.	H.M.S. Erebus	1·075	·0003127
II.	Van Diemen's Land	1·080	·0003142
III.	Montreal..........	1·074	·0003124
IV.	Cape of Good Hope	1·084	·0003153
V.	St. Helena	1·080	·0003142

TABLE III.

No. of Wheel.	I. H.M.S. Erebus.	II. Van Diemen's Land.	III. Montreal.	IV. Cape of Good Hope.	V. St. Helena.
1	·2536	·2549	·2529	·2542	·2536
2	·3032	·3058	·3055	·3055	·3065
3	·3529	·3516	·3529	·3497	·3513
4	·4058	·4088	·4078	·4052	·4071
5	·4562	·4555	·4581	·4555	·4545
6	·5055	·5071	·5042	·5055	·5058
7	·5555	·5604	·5588	·5565	·5591
8	·6071	·6071	·6071	·6097	·6081

Vertical Force Magnetometer.

The instrument used in determining the changes of the *vertical component* of the magnetic force is a magnetic needle resting on agate planes, by knife-edges, and brought to the horizontal position by weights. From the changes of position of such a needle, the changes of the vertical force may be inferred, when we know the mean inclination at the place of observation, the azimuth of the plane in which the needle moves, and the angle which the line connecting the centre of gravity and centre of motion makes with the magnetic axis. As, however, the determination of this latter constant would involve the necessity of considerable additions to the apparatus, the plan adopted has been to *adjust* the needle so that the angle in question shall be *nothing*. The centre of gravity being thus brought to some point of the magnetic axis, the changes of the vertical force are connected with the changes of position of the needle by the formula

$$\frac{\delta F}{F} = \cos \alpha \cdot \text{cotan} \cdot \theta \, d\zeta ;$$

$d\zeta$ denoting the change of angle in parts of radius, α the *azimuth* of the plane in which the needle moves, and θ the *inclination*.

Construction.—The magnetic needle is 12 inches in length. It has a cross of wires at each extremity, attached by means of a small ring of copper; the interval of the crosses being 13 inches. The axis of the needle is formed at one part into a *knife edge*, and at the opposite into a portion of a *cylinder*, having this edge for its axis, the edge being adjusted to pass as nearly as possible through the centre of gravity of the unloaded instrument. The weights by which the other adjustments are effected are small brass screws moving in fixed nuts, one on each arm; the axis of one of the screws being *parallel* to the magnetic axis of the needle, and that of the other *perpendicular* to it.

The agate planes upon which the needle rests are attached to a solid support of copper, which is firmly fixed to a massive marble base. In this support there is a provision for raising the needle off the planes, the contrivance for effecting this object being similar to that employed in the inclination instrument. The whole is covered with an oblong box of mahogany, in one side of which are two small glazed apertures, for the purpose of reading; the opposite side of the box is covered with plate glass. A thermometer, within the box, shows the temperature of the interior air; and a spirit level, attached to the marble base, serves to indicate any change of level which may occur in the instrument.

The position of the needle at any instant is observed by means of two micrometer microscopes, one opposite each end. These microscopes are supported on short pillars of copper, attached to the base of the instrument. They are so adjusted that one complete revolution of the micrometer screw corresponds to 5 minutes of arc. The micrometer head is divided into 50 parts; and, consequently, the arc corresponding to a single division is $0'{\cdot}1$.

In addition to these parts, the apparatus is provided with a brass bar of the same length as the magnet, (furnished, like it, with cross wires at the extremities, and knife-edge bearings,) for the purpose of determining the zero points of the micrometers; a brass scale, divided to 10′, used in adjusting the value of their divisions; and a horizontal needle, to be employed in determining the azimuth of the vertical plane in which the needle moves.

Adjustments.—The following are the adjustments required in this instrument:

1. The instrument being placed on its support, in a suitable position with respect to the other two instruments, the azimuth of the plane in which the needle is to move may be adjusted in the following manner. The plane is made to coincide, in the first instance, with the magnetic meridian, by means of the horizontal needle which moves upon a pivot fixed to the top of the scale. A small theodolite (or other instrument for measuring horizontal angles) is then placed on the base; and its telescope brought to bear on a distant mark. The telescope should then be moved through a horizontal angle equal to the intended azimuth of the instrument, but in an opposite direction. The base of the instrument is next to be turned, without disturbing the theodolite, until the mark is again bisected by the wires of the telescope: it is then in the required azimuth. The base should then be levelled, and permanently fixed.

2. The microscopes should now be adjusted, 1. to bring the image of the cross wires of the needle to coincide with the wires of the microscopes; and 2. to make the arc value of the interval of the wires, corresponding to one revolution of the micrometer head, exactly equal to five minutes*. These arrangements have been nearly effected in the first construction of the instrument; for the purpose of completing the adjustment, the microscopes are capable of a double motion, one of the entire body of the instrument, and the other of the object glass alone. It is manifest that these two movements are sufficient to effect both adjustments. The former is attained when the cross of wires is seen distinctly (and without parallax) at the same time that the microscope wires are exactly in the focus of the eye-piece: the latter is accomplished when the moveable wire of the microscope is made to pass over a given number of divisions of the scale, by double the number of *complete* revolutions of the micrometer head.

3. The *fixed* wires of the microscopes are then to be adjusted to the same *horizontal* line. This is effected by means of the brass needle. This needle being placed upon the agate planes, by its knife edges, and allowed to come to rest, it is manifest that the line joining the cross wires will be horizontal, provided it be perpendicular to the line joining the centre of gravity and the axis. To effect this latter adjustment, the needle (a great part of whose weight is disposed below the knife-edge) is furnished also with a small

* This adjustment is by no means a necessary one. It is sufficient for all purposes if the arc value corresponding to one revolution of the micrometer be accurately known.

moveable weight. The test of the adjustment is similar to that of the corresponding adjustment of the ordinary balance. The moveable wire of one of the microscopes being brought to bisect the cross, if the adjustment is complete, it will bisect the cross at the other extremity upon reversal; if not, the position of the needle will indicate in what manner the weight is to be moved.

A horizontal line being thus obtained, the fixed wires of the microscopes are to be adjusted to it, by moving the capstan-headed screws with which they are connected.

4. The last adjustment is that of the magnetic needle itself. This adjustment is twofold: 1. of the needle to the horizontal position; and 2. of the centre of gravity of the needle to the magnetic axis. To effect this double adjustment the needle is furnished with two moving weights, one on each arm. These weights (it has been already stated) are screws moving in fixed nuts, one in a direction parallel to the magnetic axis of the needle, and the other in a direction at right angles to it. By the movement of the former the needle is brought to the horizontal position; and by that of the latter, the centre of gravity is made to coincide with the magnetic axis. The latter part of the adjustment is tested by inverting the needle on its supports; the inclination of the needle should not be altered by this inversion when the adjustment is complete.

Observations.—In observing the variations of the vertical force with this instrument, it is only necessary to bring the moveable wire of each micrometer to bisect the opposite cross of the needle; unless in seasons of disturbance, the needle will be found at each instant to have assumed its position of equilibrium. The interval between the fixed and moveable wires, expressed in angular measure, is the deviation of the needle from the horizontal position; and the changes of the vertical force are thence obtained by multiplying by a constant coefficient.

If n denote the number of minutes, and parts of a minute, in the observed angle of deviation, the changes of the force are expressed (as in the case of the other component) by the formula

$$\frac{d\,\mathrm{F}}{\mathrm{F}} = k\,n\,;$$

in which the constant coefficient is

$$k = \cos a \operatorname{cotan} \theta \sin 1'.$$

The quantity F in the preceding formula is the product of the vertical component of the earth's magnetic force multiplied by the moment of free magnetism of the needle; or

$$\mathrm{F} = m\,\mathrm{Y}.$$

Accordingly the results thus deduced require a correction for the effects of temperature upon the quantity m. This correction is similar to that applied to the horizontal intensity; and the corrected expression of the changes of the vertical component is accordingly

$$\frac{d\,\mathrm{Y}}{\mathrm{Y}} = k\,n + q\,(t - 32)\,;$$

where t denotes the actual temperature (in degrees of Fahrenheit) at the time of observation, and q the relative change of the magnetic moment of the needle corresponding to one degree. As in the case of the other instruments, however, it is not in general necessary to apply these reductions to the individual results.

Times of Observation.

The objects of inquiry in terrestrial magnetism may be naturally classed under two heads, according as they relate, 1. to the *absolute* values of the magnetic elements at a given epoch, or their mean values for a given period; or 2. to the *variations* which these elements undergo from one epoch to another. It will be convenient to consider separately the observations relating to these two branches of the subject.

Absolute Determinations.

By the method of observation which has been suggested for the *absolute declination,* every determination of the position of the declination bar is rendered absolute. We have only to consider the varying angle between the magnetic axis of the bar and the line of collimation of the fixed telescope, as a correction to be applied to the constant angle (already determined) between the latter line and the meridian. It is manifest that if the *fixity* of the line of collimation of the telescope could be depended on, a single determination of the latter angle would be sufficient. But this is not to be trusted for any considerable period; and it will be therefore necessary, from time to time, to refer the line of collimation of the telescope to the meridian, by means of the transit instrument. This observation may be repeated *once a month,* or more frequently if any change in the position of the telescope be suspected.

In the case of the *intensity,* there is another source of error (besides that due to a change in the position of the instruments) which can only be guarded against by a repetition of *absolute* measurements. The magnetic moment of the magnet itself may alter; and the observations of intensity changes afford no means of separating this portion of the effect from that due to a change in the earth's magnetism. This separation can only be effected by means analogous to those employed in the determination of the absolute value of the horizontal intensity; and accordingly one or other (or both) of the methods proposed for this determination should be occasionally resorted to. It is desirable that this observation should be repeated *once in every month*; and more frequently, whenever the changes observed with the horizontal force magnetometer indicate, by their *progressive* character, a change in the magnetic moment of the suspended bar.

It would be easy, in theory, to devise a method by which the vertical force magnetometer might be made to serve in determining the absolute value of the vertical intensity. The means which at present offer themselves appear, however, to be surrounded with prac-

tical difficulties; and it seems safer to deduce this result *indirectly.* From the formulæ given in page 224, we have

$$Y = X \tan \theta;$$

so that if the *inclination* θ be known, and the horizontal intensity X determined in absolute measure, the vertical intensity Y is inferred.

For the purpose of observing the element θ, each observatory is furnished with an inclination instrument, the circle of which is $9\frac{1}{2}$ inches in diameter. The observation should be made in an open space, sufficiently remote from the magnets of the observatory, and from other disturbing influences; and a series of measures should be taken *simultaneously* with the two intensity magnetometers, for the purpose of eliminating the *changes of the inclination* which may occur in the course of the observation. As to the mode of observation, the best seems to be the usual one, the plane of the circle coinciding with the magnetic meridian; but for the purpose of testing the axles of the needles, and the divided limb of the instrument, it is desirable that some observations should be made in *various azimuths,* —for example, every 30° of the azimuth circle commencing with the magnetic meridian. The inclination is then inferred, from each pair of corresponding results, by the formula

$$\cot^2 \theta = \cot^2 \zeta + \cot^2 \zeta';$$

ζ and ζ' being the observed angles of inclination in two planes at right angles to one another. Where the inclination is great (as at Montreal), this method will serve to test only a limited portion of the circumference of the axle and limb. In this case the best course appears to be that pointed out by Major Sabine*, namely, to convert one of the needles, temporarily, into a needle on Mayer's principle, by loading it with sealing-wax; and to deduce the inclination, from the angles of position of the loaded needle, by the known formula of Mayer. The observations here suggested having been very carefully made, and the inclination changes eliminated in the manner above explained, the observed difference between the *mean* and the result obtained in the *magnetic meridian,* should be applied as a correction for the errors of axle and limb to all future observations made in the meridian.

These observations should be made at the same periods as those of the absolute horizontal intensity.

Variation of the Elements.

The *variations* of the magnetic elements are, 1. Those variations whose amounti s a function of the *hour angle* of the sun, or of his *longitude*; and which return to their original values at the same hour in successive days, or the same season in successive years. These, from their analogy to the corresponding planetary inequalities, may be denominated *periodical.* 2. The variations, which are either continually *progressive,* or else return to their former values in long and unknown periods; these may in like manner be denominated *secular.* 3. The *irregular* variations, whose amount changes

* *Reports of the British Association,* vol. vii. p. 55.

from one moment to another, and which observe (apparently) no law.

The *periodical* variations (with the exception of those of the *declination*) have hitherto been little studied; and, even in the case of the single element just mentioned, the results have scarcely gone beyond a general indication of the hours of maxima and minima, and of the changes of their amount with the season. The subject is nevertheless of the highest importance in a theoretical point of view. The phenomena depend, it is manifest, on the action of solar heat, operating probably through the medium of thermoelectric currents induced on the earth's surface. Beyond this rude guess, however, nothing is as yet known of the physical cause. It is even still a matter of speculation whether the solar influence be a *principal*, or only a *subordinate* cause, in the phenomena of terrestrial magnetism. In the former case, the periodical changes are to be regarded as the effect only of the *variations* of that influence; in the latter, they must be considered as its entire result, the action in this case only serving to modify the phenomena due to some more potent cause. It may be fairly hoped that a diligent study of this class of phenomena will not only illustrate this and other doubtful points in the physical foundation of the science; but also, whenever that physical cause shall come to be fully known, and be made the basis of a mathematical theory, the results obtained will serve to give to the latter a numerical expression, and to test its truth. Even the knowledge of the empirical laws of the hourly and monthly fluctuations must prove a considerable accession to science; and (as one of its more obvious applications) will enable the observer to reduce his results, as far as this class of changes is concerned, to their *mean* values.

For the complete determination of the hourly and monthly changes of the magnetic elements, a persevering and laborious system of observation is requisite. The *irregular* changes are so frequent, and often so considerable, as (partially at least) to mask the regular; and the observations must be long continued at the same hours, before we can be assured that the irregularities do not sensibly affect the mean results. Again, in a theoretical point of view, the nocturnal branch of the curves by which the periodical changes are represented is quite as important as the diurnal; and it is manifest that nothing can be done towards its determination without the cooperation of a number of observers. At each of the observatories about to be founded by the liberality of Her Majesty's Government, there will be three assistant observers placed under the command of the director; and it is intended that the observations shall be taken *every two hours* throughout the twenty-four. In order that this series of observations, which is especially destined for the determination of the periodical changes, may at the same time cast some light upon the irregular movements, it is proposed that they shall be *simultaneous* at all the observatories. The hours which have been agreed upon are the *even* hours (0, 2, 4, 6, &c.) *Göttingen mean time.* It is likewise intended that *one* observation of the twelve shall be a

triple observation, the position of the magnets being noted *five minutes before and after* the regular hour. The time of this triple observation will be 2 P.M., Göttingen mean time.

The barometer, and the wet and dry thermometers, will be registered at each of the twelve magnetic hours. No observation will be taken on Sunday.

No distinct series of observations is required for the determination of the *secular* variations. In the case of the *declination*, the yearly change will be obtained by a comparison of the monthly mean results (for the *same month* and *same hour*) in successive years. The observations of two years only will thus furnish 144 separate results, from which both the periodical and the irregular changes are eliminated; so that great precision may be expected in the final result, notwithstanding the limited period of observation. The same mode of reduction will apply to the two components of the *intensity*, provided that no change shall have taken place in the magnetic moment of the bars employed. In the latter event, recourse must be had to the *absolute* determinations for a knowledge of the secular changes.

The subject of the *irregular* movements has acquired a prominent, and almost absorbing interest, from the recent discoveries of Gauss. It has been ascertained that the resultant direction of the forces, by which the horizontal needle is actuated at a given place, is *incessantly* varying, the oscillations being sometimes small, sometimes very considerable;—that similar fluctuations occur at the most distant parts of the earth's surface, at which corresponding observations have been as yet made;—and that the instant of their occurrence is the same everywhere. The intensity of the horizontal force has been found subject to analogous perturbations.

For the full elucidation of the laws of these most interesting phenomena, it is of the first importance that the stations of observation should be separated as widely as possible over the earth's surface, and that their positions should be chosen near the points of maxima and minima of the magnetic elements. This has been in a great measure accomplished as regards the observatories about to be founded by Her Majesty's Government. The stations are wide asunder in geographical position, and they are in the neighbourhood of points of prominent interest in reference to the isodynamic lines. The results of observation at these stations will soon testify whether the shocks to which the magnetic needle is subject, are of a local or of a universal character as regards the globe; and in either event we may expect that they will furnish information of great value (in reference to a physical cause) as to the magnitude of the phenomena in different places, and the elements on which it depends.

In the observations destined to illustrate these phenomena, it is proposed to follow, as nearly as possible, the plan laid down by Gauss. One day in each month, namely, the *last Saturday*, will be devoted to simultaneous observations on this system*; the observations commencing at 10 P.M. of the preceding eve (Göttingen mean time), and continuing through the 24 hours.

* For the details of the arrangements of the "*term observations*," see the translation of Gauss's memoir in Taylor's Scientific Memoirs, vol. ii. part v.

XXXIII. *Notices respecting New Books.*

Rara Mathematica; *or a Collection of Treatises on the Mathematics and Subjects connected with them: from ancient inedited Manuscripts. Edited by* James Orchard Halliwell, *Esq., F.R.S., F.S.A., &c. of Jesus College, Cambridge.* London, Parker; Cambridge, Deightons.

WE called the attention of our readers to the first number of this work at the time of its publication. The work is now brought to a close; but more abruptly than was originally intended, owing to the increase of materials pointing out to the editor the necessity of a different plan. He has, therefore, resolved upon publishing a history of early English mathematics, by which means he will be better able to classify the subjects; whilst he prints the manuscripts themselves, or such parts of them as are relevant to the subject, in the form of illustrations. This, indeed, appears to us to be, upon the whole, a better plan than that which he first adopted, and we look forward with considerable interest to the completion of his arduous undertaking. That there are ample materials amongst the manuscripts in our private and public libraries, we can entertain no doubt; and we think it a matter of great advantage that these treasures of the science of the middle ages should be investigated by one who is at the same time a mathematician and an antiquary. This combination of tastes and talents in the same mind is a rare occurrence in this country; though on the continent we find occasional instances of it, as in the case of Delambre, Libri, Chasles, and a few others.

We purpose here to state generally the nature of the treatises now brought together by the persevering industry and just discrimination of the editor, making as we pass along a few remarks upon the historical information contained in some of them.

I. *Johannis de Sacro-Bosco Tractatus de Arte numerandi.*

This is printed from a MS. in the editor's own library, purchased at the sale of the library of the Abbate Canonici of Venice.

Who Sacro-Bosco was, is by no means determined; though, upon very slender grounds, he is commonly considered to be a native of Halifax. His work, however, was very popular during the middle ages, and copies of it in MS. are of very frequent occurrence, sometimes with, and often without, his name. In the Ashmolean library there is an English translation of it (396,), and the only one of which we have any knowledge. It is singular that it should not have been printed in the fifteenth or sixteenth century, and is only to be accounted for by its being superseded in the public estimation by other and better treatises. It is, as an element of history, of considerable interest, particularly in respect to the clumsy ingenuity which runs through it—of which we may instance the rule for progressions (p. 18, 19, *Rara.*)

This treatise has often been quoted on account of the following passage: "Hanc igitur scientiam numerandi compendiosam edidit

nomine Algus, unde algorismus nuncupatur, vel ars numerandi, vel introductio in numerum." And it is remarkable that the same statement is supported by other authorities. Johan. de Norf. adds, " Rex quondam Castelliæ." However, the tract being now before the public, we need say no more concerning it, as every one interested in the subject can consult it at his leisure.

II. *A method used in England in the fifteenth century for taking the altitude of a steeple or inaccessible object.*

This is a tentative method adapted to the observer being destitute of the necessary surveying instruments. English mathematicians were then, as the editor observes, " skilful in the application of the quadrant, and all other known scientific instruments." We rather think it a contrivance such as we have said than the device of the more ignorant classes. Perhaps it may partake of both.

III. *A treatise on the numeration of Algorism:* from a single leaf of vellum found loose in an old MS. on astronomy, in the editor's possession. It is a treatise on what we properly call "numeration," or the local value of the digits composing a number. It is intended, evidently, as the introduction to a treatise on arithmetic in English, and probably formed part of one.

IV. *A treatise on the properties and qualities of glasses for optical purposes, according to the making, polishing and grinding of them: by William Bourne.*

Bourne is well known to readers of works of the sixteenth century. He was a skilful navigator, and possessed a very inventive mind. Some, but a very incomplete account of him, may be seen in Dr. Hutton's Mathematical Dictionary.

This tract it appears was drawn up at the request of "Elizabeth's great minister," and is inscribed to him; and Burleigh's object seems to have been to ascertain something relative to the art of seeing hidden things by means of glasses,—a favourite belief of that time, arising probably from the pretensions of Dee and Digges to the possession of such power. Bourne gives a plain and succinct account of the properties of reflecting and refracting mirrors, in the main, derived evidently from experiment. With one or two exceptions, the descriptions are correct. His lens of a foot diameter, however, is merely a conjecture of what may be possible, and evidently had never been made or seen by him.

He does not give the slightest hint of combining two refracting mirrors, so as to form a telescope. This was reserved for Galileo. Still he proposes the employment of a concave reflector to increase the magnitude of the image given by a convex refracting lens. A reversal of this process would have led to the most important results, and might have long anticipated the suggestion of Mersenne and the invention of Gregory.

There is one point exceedingly curious in this treatise. He states the *foci of heat and light are not identical*; a circumstance which we believe had never been suspected by philosophers till Sir William Herschel established it in the Philosophical Transactions for 1800.

When we consider the want of any accurate means of estimating

the intensity of heat and light that Bourne had to contend with, and the then prevalent idea that heat and light (so far as the sun was concerned) were necessarily in the same ratio, and essentially coincident in their place of action, we cannot but wonder at the opinion being hazarded by a man of so sensible and modest a character: and we are obliged to consider him an extraordinary person, even though he mistakes (probably an error of the hand rather than the mind) the side of the focus of light on which the focus of heat falls.

V. *Johannes Robyns de Cometis.* A curious specimen of the rhodomontade of the astrologers of the middle ages. It appears, however, that Robyns had read Cicero, for part of this preface is plagiarised from him.

VI. *Two tables, one showing the time of high water at London Bridge, the other the duration of moonlight.*

An extract from the first of these tables was given by Mr. Lubbock in the Phil. Trans. for 1837, p. 103, of which the editor of this collection was ignorant at the time his MS. was sent to press (Rara, Corrections). It is a very curious document, showing, as Mr. Lubbock remarks, that the *time of high water at London Bridge was then "more than an hour later than at present*;" and, therefore, that immense interruption to the progress of the tide in the River Thames has been removed in the course of six centuries. We ought not, however, to forget that great difficulties then existed in determining the actual time of high water, though there can be little doubt that the approximation was as correct as such circumstances would allow, and that at most the error could not be more than three or four minutes.

VII. *A treatise on the Mensuration of heights and distances; from a MS. of the* 14*th century.*

This treatise contains several ingenious methods of mensuration, or rather of right-angled trigonometry, and is very curious as explanatory of the practice of the times. The quadrant had then the form of a square, and the unknowns were all determined by means of similar triangles. Though the MS. is of the 14th century, it is too full of methods adapted to different exigencies to indicate the general matter of it being then of a recent date. The methods themselves are all independent of any function of the angle observed; and hence most probably this MS. is only a copy of one of a much older date, and antecedent to the introduction of the Arabian trigonometry into Europe. Being in English, it shows at least that Englishmen then understood the doctrine of similar triangles.

This "tretis" is, however, valuable on account of the very curious editorial note, at pp. 56, 57, attached to the author's etymology of the word "Geometri," from *geos* and *metros*. It consists in an extract from a very ancient MS. in the King's library relative to the time when geometry as a science was first introduced into England:

> "The clerk Euclyde on þis wyse hit fonde
> Thys craft of gemetry yn Egypte londe
> Yn Egypte he tawghte hyt ful wyde
> Yn dyvers londe on every syde.

> Mony erys afterwards y vnderstonde
> Gher þat þe craft com ynto þys londe.
> Thys craft com ynto England, as y ghow say,
> Yn tyme of good kyng Adelstone's day."

"This notice," remarks Mr. Halliwell, "of the introduction of Euclid's Elements into England, if correct, invalidates the claim of Adelard of Bath, who has always been considered the first that brought them from abroad into this country, and who flourished full two centuries after the good kyng Adelstone."

We would also refer to the curious fact that the celebrated translation of Campanus is nothing more than Adelard's with a commentary; and it is curious that this should not have been noticed till very recently, and then by three persons independently of each other;—Tiraboschi; the anonymous author of "Geometry," in the Penny Cyclopædia; and by the late Mr. Charles Butler of Cheam. For reasons given by Mr. Halliwell, it would almost seem that the commentary as well as the translation attributed to Campanus, is also due to Adelard.

VIII. *An account table for the use of merchants: from a manuscript of the* 14*th century.*

This table will probably be a puzzle to most of our readers; and it is doubtful whether, from the editor's remark on the subject, he has not himself mistaken its exact character. It is, however, neither more nor less than a *picture* of a merchant's abacus or counting *board.* The ciphers in the several squares represent holes bored to receive pegs or pins, which mark the numbers of poundes, schillynges, penyes, and ferthynges in any sum of money. There are holes for 1, 2, 3 ferthynges at the upper part of the right side of the board; for 1, 2, ... 11 penyes from right to left along the top; below and parallel to these for 1, 2.... 11 shillynges, and vertically down the left side for 12.... 19 shyllynges; whilst in the other part of the board is a table for poundes, which proceeds according to the *decimal scale* to 10^{10}, with nine horizontal columns, of holes for inserting pegs to express any one of the nine digits as the coefficient of any one of the powers of 10. This table would therefore express any number less than 10^{11}. The vertical columns are headed in Roman letters as annexed, and below them we put their values in modern notation:

...	..	..	..	.	.	.			
X	M	C	X	M	C	X	M	C	X
10^{10}	10^{9}	10^{8}	10^{7}	10^{6}	10^{5}	10^{4}	10^{3}	10^{2}	10^{1}

and it is remarkable that 10^0 or the units' column is left without any heading at all. We may add also that the numbers 1, 2,.... 19 in the fractional part of the table are written in the manuscript in the common figures of the time, whilst the decimal places are expressed by Roman letters.

This is perhaps one of the most curious MSS. yet discovered for the purposes of arithmetical history, as it shows, what has never been suspected by inquirers into the subject, that the abacus was used in a form very nearly resembling the common method of modern calcu-

lations. But this is not its only value, for it fully explains some early MSS. on arithmetic; such as that known as the Metz MS., (343 Arundel,) Chartres, (see *Chasles, Aperçu His. de Méth. en Geom.*, p. 467) and one or two others. Into this subject it would, however, be impossible to enter in this place, though possibly some of our correspondents may be led to consider this matter at greater length*.

IX. *Carmen de Algorismo.* This song is, by a MS. in the British Museum, and by another in the French King's library, attributed to Alexander de Villa Dei. Its chief value is its showing the opinion that was currently entertained respecting the Indian origin of the modern notation for numbers; and as MSS. of it are very numerous it has often been quoted as an authority on that head.

Perhaps there are no MSS. of the same description which has a greater number of variations than those of this Carmen. There is one in the Ashmolean, which contains from 20 to 30 lines more than that here printed from, which is that in the author's library. It is a general principle amongst antiquaries in cases of this kind to consider the lines which one MS. has in excess over another as interpolations; except, indeed, collateral evidence should support the authenticity of the so suspected passages. As we have not collated the Oxford MS. with this, we can of course offer no opinion on the subject, and merely make the remark as a suggestion to those who may feel disposed to assent to the superior authority of the Oxford as compared with Mr. Halliwell's manuscript.

X. *Prefatio Danielis de Merlai ad librum de naturis superiorum et inferiorum.*

Its value is of the same kind as the last.

XI. *Proposals for some inventions in the mechanical arts; from the Lansdowne Collection, dated* 1583.

Probably by William Bourne.

XII. *The preface to a calendar or almanac for the year* 1430.

From the opening address and date, it would seem to have been written for the use of the queen of Henry VI. It is a fair sample of such prefaces, being merely a set of directions for the use of the dominical letters, &c., and, indeed, some parts of it are almost literally identical with corresponding ones in Chaucer's preface to his tract on the Astrolabe.

XIII. *Johannis Norfolk in artem progressionis summula.*

A curious specimen of ingenuity to evade the difficulties which a more enlarged view of the nature of numbers could alone remove. It also contains the passage already quoted on a former page: "Rex quondam Castelliæ."

To the work are appended two tracts, one on the *Numeral contractions found in some manuscripts of the Treatise on Geometry by Boetius;* and the other, of *Notes on Antient Almanacs.*

* We have seen some of the earlier sheets of a work by Mr. Davies, in which this view is taken and contended for: but as the work is not, that we know of, published, we can give no account of it here.

In respect of the first of these, it is sufficient to say, that Mr. Halliwell offers reasons for believing that the passage itself in Boetius is an interpolation, and therefore of no higher authority than the age of the oldest MS. in which it is found. It also corrects an opinion given by M. Chasles, that the *sipos* of the earlier MSS. was the same with the *celentis*, both signifying either the figure or the number nine. Chasles has since found other MSS. which show the correctness of Mr. Halliwell's view, viz. that *celentis* was alone used to designate *nine* and *sipos* the *cipher*. Into this subject we cannot here enter further, but merely refer to the work itself, and to two notices by Chasles in the *Comptes Rendus*, May 1838, and Jan. 1839, as well as to Vincent's paper in Liouville's *Journal de Math.* June 1839. These writers offer strong reasons for believing that both the figures used and their local values, had an origin different from, and quite independent of, that commonly assigned to them.

The notes on early almanacs was first published in the "Companion to the Almanac" for 1839. It contains some curious matter, but we would add that the clog-almanacs vary exceedingly in their notation, and that they are very numerous in collections, the Ashmolean containing nearly half a score, most of them in good preservation. The author might also have referred to Hone's Every-day Book for a good account of them.

In conclusion, we would express our entire approbation of this curious and valuable collection of early treatises on mathematics, and our anxiety to see the author's more extended plan completed. Despoiled as our MS. libraries were by the Vandals of the Reformation, the devoted enthusiasm of collectors has still saved a vast mass from the general wreck; and we trust that men equally devoted will always be found to estimate their value, and deduce from them the facts of which they contain the most conclusive evidence. Our antiquaries should always recollect that the history of science is the history of the human character and of the state of social and moral life.

XXXIV. *Intelligence and Miscellaneous Articles.*

SEPARATION OF LIME AND MAGNESIA. BY DÖBEREINER.

If anhydrous chloride of magnesium be heated in the air, it absorbs oxygen and gives off chlorine. This decomposition, that is to say, the conversion of chloride of magnesium into magnesia, is more quick and complete when chlorate of potash is used instead of air as an oxidizing agent. This property renders the separation of lime and magnesia very easy. A mixture or compound of these two bodies, dolomite for example, is to be dissolved in hydrochloric acid; the solution is to be evaporated to dryness; the residue of the evaporation is to be heated in a platina capsule, till it ceases to yield hydrochloric acid, and then there are to be gradually added to the mass heated to low redness, small portions of chlorate of potash, till the disengagement of chlorine ceases. The residual mass is then a mixture of magnesia, chloride of calcium, and chloride of

potassium, which are readily separated by treating the mixture with water, which dissolves the chloride of potassium and of calcium, while the magnesia is left; from the mixture of chloride of potassium and of calcium, the lime is precipitated by carbonate of soda.—*Journal de Pharm.* July 1839.

MODE IN WHICH SOME SALTS ACT IN HYDROGEN GAS. BY WÖHLER.

Some researches on the peculiar mode of composition of mellitic acid, have caused me to observe that the salt of silver of this acid, exposed to pure hydrogen gas at 202°, very quickly changed from its white colour to black, and was afterwards soluble in water and imparted a deep red colour to it. During this reaction a little water was formed, and it lost oxygen, equal to half the weight of that contained in the oxide. The brown solution of the altered salt was strongly acid, and deposited after some time bright metallic silver, and became colourless; it then contained merely the common colourless salt dissolved in free acid.

This circumstance indicated with great probability, that by the action of the hydrogen upon this salt, the silver was reduced to the state of protoxide, a supposition which was completely confirmed by examining into the modes in which other salts of silver exist, and the existence of a protoxide of silver was satisfactorily determined.

Of some other salts of silver which I carefully examined with this view, the nitrate was that which evinced the most evident alteration. When exposed at 212° to a current of dried hydrogen gas, it becomes throughout the mass, and very quickly, of a deep brown colour. The action even begins at common temperatures, as it does with the mellitate. The mass is then a mixture of nitrate of protoxide and free nitric acid. Half of the oxygen of the oxide of silver is disengaged in the state of water, from two atoms of the salt of the deutoxide; there is formed $Ag^2 O$, which remains combined with half of the acid, whilst the other half is set free. Water dissolves the free acid, and as soon as the principal part of this is removed, the protosalt begins to dissolve in the pure water with a deep red colour. In the dry state this salt is a powder of a deep brownish black colour. When heated it decomposes with a much weaker detonation than the white deutosalt. It then leaves 76 per cent. of metallic silver, a quantity which ought to remain according to the formula $Ag\ C^4\ H^4\ O^4$.

If the red solution of the protosalt be boiled, it gradually decomposes with a slight disengagement of gas; it becomes opalescent and of a peculiar yellowish green colour; afterwards it deposits metallic silver, and becomes colourless. The brown protosalt dissolves in ammonia also with a very deep yellowish red colour. When heated the solution undergoes a decomposition similar to the preceding. Sometimes the sides of the vessel are covered with a brilliant metallic coating almost of a golden colour, and which, like very finely divided gold, is transparent and of a fine green colour.

Potash precipitates a perfectly black heavy powder from the red solution of the protosalt, which is rendered colourless at the same time. This black powder is obtained also by the direct decomposition of the dry salt by means of a solution of potash; this precipitate remains black after drying; by pressure it becomes of a deep metallic lustre, and by heat is reduced to metallic silver, evolving oxygen. The black colour seems to indicate that it is pure protoxide of silver; but this supposition does not always depend on the colour, for this powder might also be, consistently with its properties, an intimate mixture of deutoxide of silver and metallic silver, to which the protoxide may have given rise at the moment of its separation. It is also decomposed by the acids into metal and deutosalts, and ammonia exerts a similar action. Hydrochloric acid converts it into a brown substance, which is a chloride corresponding with the protoxide or perhaps merely a mixture of silver and common chloride of silver; this substance is also obtained in the state of a brown, curdy precipitate, which speedily subsides, by precipitating the red solution of protonitrate of silver by hydrochloric acid; it acquires the metallic lustre by pressure. When heated to the temperature at which chloride of silver fuses, it becomes merely a yellow mass, and is a mixture of silver with the common chloride. When treated with ammonia, or even with concentrated solution of the hydrochlorate, the brown chloride is decomposed immediately into chloride which is dissolved, and into metallic silver which remains.

Oxalate of silver when exposed at 212° to the action of hydrogen gas, becomes of a bright yellow tint; but the decomposition seems to remain only partial at this temperature. It became brown at 284°; but it soon afterwards produced a very loud explosion. Succinate of silver becomes lemon-yellow at 212° in hydrogen gas. At a higher temperature, half of the succinic acid sublimed. The protosuccinate of silver thus formed is insoluble in water. Pure deutoxide of silver is reduced to the metallic state precisely at 212° in hydrogen gas.—*Journal de Pharm. Juillet* 1839.

ON CUBEBIN. BY MM. CAPETAINE AND SOUBEIRAN.

We have discovered on cubebs a peculiar matter, to which we give the name of *Cubebin*. Although M. Monheim has already applied this word to a product which he obtained in his experiments on cubebs, it is certain that he did not procure the true cubebin, as may be seen by the properties which he assigns to it. The cubebin of M. Monheim is greenish, has an acrid taste, melts at 68°, boils at 86°, and then is partly volatilized, whereas the true cubebin is white, insipid, inodorous, and decomposes before it fuses.

The process which we found to succeed best in obtaining cubebin, consists in pressing the marc which remains after the preparation of the volatile oil of cubebs to make an alcoholic extract of it, and to treat this extract with a solution of potash, as proposed by Poulet for the preparation of pipesin. The cubebin is to be washed with a little water, and to be purified by crystallization repeatedly from alcohol.

Cubebin is white, insipid, inodorous. It occurs in groups of small acicular crystals. At 392° *in vacuo*, it loses no weight. It is not volatile, and is scarcely soluble in water; cold alcohol dissolves but a small quantity of it; at 53°, 100 parts of absolute alcohol dissolved but 1·31 part: alcohol of 82° dissolved 0·70, but when boiling both dissolved so much that on cooling the liquor became a mass. At 53°, 100 parts of æther dissolve 3·73 of cubebin; it is more soluble in it when hot; it is also soluble in acetic acid, in the volatile and fixed oils.

Concentrated sulphuric acid renders it of a deep red colour. This substance was analysed by means of oxide of copper, it being previously dried in a dry vacuum at 322°; it was found to consist of

Hydrogen	5·56	
Carbon	68·29	
Oxygen	26·25	100·

which are equivalent to nearly

Eight equivalents of	hydrogen	8	or	5·33
Seventeen ,,	carbon..	102		6·8
Five ,,	oxygen..	40		26·67
		150		100·

Cubebin is neutral, and does not appear susceptible of forming any compound from which its true atomic constitution can be inferred. In this respect it is a substance of little interest; but the following two consequences result from its composition: first, that it differs essentially from the crystalline matter of black pepper in several respects, and especially in its composition, since it contains no azote; that it is not derived from the volatile oil of cubebs, this, according to our experiments, containing hydrogen and carbon in the atomic relation of 5 to 8, as the oil of turpentine does.—*Journal de Pharm.*, *Juillet* 1839.

ANHYDROUS PHOSPHORIC ACID. BY M. MARCHAND.

A small porcelain capsule is to be placed on a stand, in a large porcelain vessel; some pieces of dry phosphorus are to be put into the small capsule, and a tubulated glass receiver is to be placed over it; a cork containing two glass tubes is to be inserted into the tubature; of those tubes, one is large and almost reaches the small capsule, and it is to be fitted with a cork; the other tube is narrow, and is to be bent at an angle externally. The narrow tube is to be connected with an apparatus, from which dry oxygen gas is disengaged; a retort in which chlorate of potash is heated is to be preferred. It is more convenient to convey a current of gas from a gasometer, and to dry it perfectly by chloride of calcium and sulphuric acid in the potash apparatus of Liebig. At first oxygen is to be passed through the receiver to expel the atmospheric air from it; the phosphorus is then to be inflamed with hot iron rod passed through the larger straight glass tube. When all the phosphorus is burnt, more is passed through this tube into the small capsule. The retort may also be readily changed when all the chlorate of

potash is decomposed. When the glass receiver becomes too hot, the operation is to be discontinued for some time to cool it, otherwise it will infallibly be broken. In this manner a considerable quantity of nearly pure anhydrous phosphoric acid may be prepared in a short time, with a quarter of a pound of phosphorus. M. Marchand obtained more than half a pound of the anhydrous acid. When the combustion is properly conducted, scarcely any vapour escapes. The flocks of phosphoric acid which attach to the receiver and the capsule are to be speedily removed, and preserved in well-stopped bottles.—*Journal de Pharm., Juillet* 1839.

CINNAMIC AND FORMIC ÆTHERS. BY M. MARCHAND.

Cinnamic æther is very readily obtained by distilling a mixture of 4 parts of absolute alcohol, 2 parts of cinnamic and 1 part of hydrochloric acid. As its boiling point is very high, that which first comes over may be recohobated several times; there remains an oily liquid in the retort after about three-fourths of the liquor have been distilled; this is to be shaken with water, and after decantation it is to be distilled on oxide of lead. The pure æther distils when the temperature reaches 500°. It is limpid, with a slightly yellowish tint, and its specific gravity is 1·13 at about 54°; its smell and taste are aromatic and æthereal, and resemble those of cinnamon; it boils at 500°; it is very little soluble in water, but very soluble in æther and in alcohol. In contact with potash, it decomposes very readily into æther and cinnamic acid, which combines with the potash. It appears to form a peculiar compound with ammonia. Fuming nitric acid does not act, or acts but very feebly upon it. It yielded by analysis

Hydrogen	6·711
Carbon	75·362
Oxygen..............	17·927

and according to M. Marchand its atomic constitution is

Twenty-one	equivalents of	hydrogen	149·754
Twenty-two	,,	carbon	1681·570
Four	,,	oxygen	400·000
			2231·324

Whence it follows, says M. Marchand, that cinnamic æther is formed of 1 atom of anhydrous cinnamic acid $H^{14}\ C^{18}\ O^3$, and 1 atom of oxide of æthule. It is stated that similar results had previously been obtained by M. Herzog.

Formic Æther.—The preparation and properties of this substance are well known. That which M. Marchand analysed had been perfectly dried over chloride of calcium; its boiling point was about 130°. The specific gravity of its vapour according to M. Liebig is 2·57. By analysis it appeared to consist of

Twelve	equivalents of	hydrogen	74·877	or 8·02
Six	,,	carbon	458·622	49·13
Four	,,	oxygen	400·000	42·85
			933·499	100·

These results, M. Marchand remarks, agree with the formula admitted for formic æther [acid ?], and are confirmed by the specific gravity.—*Journal de Pharm., Juillet* 1839.

ANALYSIS OF HAILSTONES. BY M. GIRARDIN.

In a letter addressed to M. Arago, and read at the Academy of Sciences, on the 22nd of April last, M. Girardin gives the results of the analysis which he has made of hailstones collected in the month of February preceding; the following details are extracted from the latter.

The hailstones, collected with the requisite precautions, were introduced into a bottle washed with distilled water, they weighed about 500 grains; the hail readily melted, and the liquid had the appearance of water into which a few drops of milk had been suffered to fall; it was turbid and whitish. Gradually there were formed in it a considerable abundance of white and very light flocks, which soon formed into one cloudy mass, and deposited at the bottom of the vessel. The next morning, the liquid was perfectly limpid.

A portion of the water, whilst it was still whitish and milky, was put into a glass, and a few drops of nitrate of silver were added to it. The glass, stopped with paper, was placed in the dark and allowed to remain for twelve hours. The addition of the reagent produced no apparent effect, and the liquid preserved its original aspect without becoming coloured. On being afterwards put into a place brilliantly lighted, it became almost suddenly reddish; then in about an hour, it assumed a brown colour, and deposited grayish flocks, and brilliant white pellicles were at the same time formed on the surface. The flocks, separated from the liquid, were calcined in a small glass tube; they emitted a smell of burnt animal matter, and reddened litmus paper was rendered blue by exposure to them. There remained at the bottom of the tube a grayish powder, which was a mixture of charcoal and metallic silver.

The greater portion of the hailstone water was evaporated while turbid and milky, cautiously in a platina capsule. During the evaporation, no trace of ammonia was perceptible; the residue was of a yellowish brown colour, but the quantity was so small that the weight could not be determined. A similar evaporation having been made in a glass tube, the residue was heated to incipient redness. It exhaled during calcination a very sensible odour of ammonia, and reddened litmus paper was rendered blue by it: there remained a trace of charcoal at the bottom of the tube.

Hailstone water filtered and clear became slightly turbid with oxalate of ammonia, and more so with nitrate of barytes, and nitric acid did not restore the transparency. No other reagents produced any effect. In the small quantity of this hailstone water which I had, I could not discover the existence of nitric acid.

It follows from what has been stated that the hailstones examined contained a considerable portion of organized and azotized matter, a sensible quantity of lime and sulphuric acid, but no sensible trace of ammonia.

Several chemists have directed their attention to the existence of an organic substance and saline matters in the atmosphere. Their experiments have unquestionably proved, that rain water in falling through the atmosphere carries with it in solution into the earth, ammoniacal salts, calcareous salts, and a flocky matter, which is without doubt the origin of the deleterious principles which are designated by the term *miasmata*. Hitherto, however, no one has stated the existence of this organic matter in hailstones.—*Journal de Pharm.*

VERATRIC ACID. BY M. MERCK.

I have convinced myself, observes M. Merck, by repeated observations that the seeds of cevadilla contain, besides the bodies already known, several new and extremely interesting substances; and I have obtained a quantity of one of them in a state of purity. Professor Schrötter has analysed it in M. Liebig's laboratory, and has determined its formula, and his experiments prove that it is a new and peculiar acid. This acid, which I shall call *veratric acid*, is very readily obtained by treating cevadilla, in the manner directed by M. Couerbe, with alcohol and sulphuric acid, for preparing veratria; hydrate of lime is to be added to the alcoholic tincture, and the alcohol is to be distilled from the filtered liquor. The watery liquor which remains with the residue on the separated veratria, then contains the new acid in combination with lime, and it is requisite only to supersaturate it with sulphuric acid to separate the veratric acid, which, if the liquor is sufficiently concentrated, crystallizes in a few hours. Sometimes it is necessary to evaporate the mother waters to one half, and to heat them for some time after the addition of the sulphuric acid, in order to separate the sulphate of lime; the concentration, however, ought not to be carried too far, because the viscidity of the liquor then renders the crystallization of the acid difficult. The crystals are completely purified by washing them repeatedly with cold water, dissolving them in boiling alcohol, and treatment with purified animal charcoal. In this condition they are colourless acicular crystals or tetrahedral prisms, according as they are obtained from a concentrated or dilute solution. This acid reddens moist litmus paper, it is soluble in alcohol, and much more so when hot than cold; it is insoluble in æther. In cold water it is but little soluble, in hot water it is more readily dissolved, and on cooling it is deposited in the form of a white crystalline powder. With the alkalies it forms compounds, which are soluble both in water and alcohol; its compounds with potash and soda are crystallizable, not deliquescent but very soluble in water. The solutions of nitrate of silver and acetate of lead produced in the concentrated solution of veratrate of ammonia white precipitates, which dissolve completely on the addition of water or of alcohol.

Fuming sulphuric acid and nitric acid of 40° B. do not appear to decompose this acid; if on the contrary some crystals be put into a mixture of sulphuric and nitric acid, they become, after a short time, of a fine yellow colour.

When cautiously heated on platina foil by a spirit lamp, the cry-

stals lose their water, become of a dull white, fuse into a colourless liquid, and evaporate without carbonizing. If this experiment be made in a glass tube, the vapour condenses in a crystalline state in the cooler part of the tube. A slip of moistened litmus paper, placed in the tube during sublimation, is strongly reddened. The property of subliming is the only one which this new acid has in common with that which MM. Pelletier and Caventou found in the seeds of cevadilla, and which ought not therefore to be confounded with the acid which I have discovered.

Professor Schrötter's analysis of this acid gives

Hydrogen	5·49
Carbon	59·95
Oxygen	34·56——100·

The atomic weight of this acid (anhydrous) is 2184·2, and that of the crystallized acid 2296·7; the formula of the first is $H^{18} C^{18} O^{7}$, and that of the second $H^{18} C^{18} O^{7} + H^{2} O$.

Journal de Pharm., Mai 1839.

GAUSS ON THE THEORY OF MAGNETISM.

In our theory it is assumed that every determinate magnetized particle of the earth contains precisely equal quantities of positive and negative fluid. Supposing the magnetic fluids to have no reality, but to be merely a fictitious substitute for galvanic currents in the smallest particles of the earth, this equality is necessarily part of the substitution; but if we attribute to the magnetic fluids an actual existence, there might without absurdity be a doubt as to the perfect equality of the quantities of the two fluids.

In regard to detached magnetic bodies (natural or artificial magnets), the question as to whether they do or do not contain a sensible excess of either magnetic fluid might easily be decided by very exact and delicate experiments.

In case of the existence of any such excess in a body of this nature, a plumb-line to which it should be attached should deviate from the true vertical position in the direction of the magnetic meridian.

If experiments of this kind, made with a great number of artificial magnets and in a locality sufficiently distant from iron, never showed the slightest deviation, (which we should rather expect,) the equality of the two fluids might with the highest degree of probability be inferred for the whole earth; though without wholly excluding the possibility of some inequality.—*Taylor's Scientific Memoirs*, vol. ii. (Part vi.) p, 228.

The Provincial Meeting of the Geological Society of France will this year be held at Boulogne, and will commence on the 8th of September.

SCIENTIFIC BOOKS.

Just published.

Scientific Memoirs, translated from Foreign Transactions and Journals: edited by Richard Taylor, F.L.S.—Part VI. 6 Plates,

containing Melloni on the Polarization of Heat.—Gauss's General Theory of Terrestrial Magnetism.—Gauss on a new Instrument for observing the Intensity of Terrestrial Magnetism.—Weber on the use of the Bifilar Magnetometer.—Schleiden on Phytogenesis. The translations of the Memoirs on Terrestrial Magnetism have been made under the direction and revisal of Major Sabine, Professor Lloyd, and Sir J. F. W. Herschel, with a view to the main object of the Antarctic Expedition under Capt. J. C. Ross.

The Philosophical Transactions of the Royal Society for 1839, Part I.

Faraday's Experimental Researches in Electricity, with 8 Plates. 8vo.

The Annals of Natural History, No. 21.

The Report of the Eighth Meeting of the British Association for the Advancement of Science, held at Newcastle in August 1838.

In the press. The Rev. Dr. Pye Smith's Congregational Lectures on the Relation between the Holy Scriptures and some parts of Geological Science.

METEOROLOGICAL OBSERVATIONS FOR JULY, 1839.

Chiswick.—July 1—3. Fine. 4, 5. Very fine. 6. Sultry. 7. Hot: thunder and much lightning at night, accompanied with unusually little rain. 8. Very fine. 9—11. Fine. 12. Cloudy: slight rain. 13. Very fine. 14. Very fine: rain. 15, 16. Fine. 17. Slightly overcast: thunder and heavy rain at night. 18. Showery: windy. 19, 20. Very boisterous. 21, 22. Fine. 23. Showery. 24. Rain: fine. 25. Overcast. 26. Very heavy rain. 27. Heavy thunder showers. 28. Cloudy. 29. Fine. 30, 31. Rain.

Boston.—July 1. Cloudy. 2. Cloudy: rain A.M. 3. Cloudy. 4. Fine. 5. Cloudy. 6. Fine. 7. Cloudy. 8. Fine: rain, with thunder and lightning A.M.: rain P.M. 9. Fine: rain, with thunder and lightning P.M. 10, 11. Fine. 12. Fine: rain P.M. 13. Fine. 14. Fine; rain P.M. 15—17. Fine. 18. Cloudy: stormy, with rain P.M. 19. Stormy. 20—23. Cloudy. 24. Cloudy: rain early A.M. 25. Cloudy. 26. Cloudy: rain P.M. 27. Rain: rain early A.M.: thunder and lightning P.M. 28. Fine: rain early A.M. 29. Fine: rain P.M. 30. Cloudy: rain early A.M.: rain P.M. 31. Cloudy: rain early A.M.

Applegarth Manse, Dumfries-shire.—July 1, 2. Very fine summer days: clear sky. 3. Very hot. 4. Warm, but with a pleasant breeze. 5. The same: getting cloudy P.M. 6. Slight showers all day. 7. Rain. 8. Slight showers A.M.: cleared and was fine. 9. Showery. 10. Wet and stormy nearly all day. 11. Warm but cloudy: very wet P.M. 12. Heavy rain during night: showery: flood. 13. Fine bracing air. 14. Showery A.M.: cleared up noon: cloudy P.M. 15. Showery A.M.: cleared, and was fine. 16. Remarkably fine day. 17. Cloudy and threatening, and electrical. 18. Frequent heavy showers and high wind. 19. Stormy day: rain nearly throughout. 20. Slight showers all day: flood. 21. Pleasant bracing air: getting cloudy P.M. 22. The same: a very slight shower. 23. Showery all day. 24. Showery A.M.: cleared up P.M. 25. Very fine day: air warm and genial. 26. The same: sultry P.M.: thunder. 27. Cooler: wind northerly: cloudy P.M. 28. Remarkably fine day. 29. Showery all day: distant thunder. 30. Fair and pleasant. 31. Slight shower A.M.

Sun 25 days. Rain 17 days. Thunder 2 days.

Wind southerly 23 days. Northerly 5 days. Westerly 1 day. Easterly 2 days.

Calm 14 days. Moderate 7 days. Brisk 4 days. Strong breeze 3 days. Boisterous 3 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. July.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		9 a.m.	9 p.m.									
1.	30·312	30·315	30·215	29·81	30·23	30·25	55·2	55·5	48·6	65	45	52	59	60	N.	NE.	calm	NW.	...	...	...	...	46
2.	30·308	30·307	30·220	29·76	30·27	30·28	56·9	63·3	51·8	70	51	59	65	59	W.	NE.	calm	NW.	...	...	...	...	50
3.	30·308	30·304	30·224	29·78	30·26	30·21	61·3	64·0	53·5	67	50	58	64½	53	N.	NE.	calm	W.	...	...	·05	...	53
☾ 4.	30·228	30·204	30·178	29·66	30·18	30·18	63·9	66·7	57·0	71	56	65	62	58	NE.	NE.	calm	SW.	...	...	...	...	55
5.	30·208	30·191	30·097	29·60	30·12	29·98	62·8	73·8	57·8	72	52	60	65	60½	E.	SW.	calm	SSE.	...	...	...	...	58
6.	30·040	29·925	29·901	29·39	29·80	29·63	66·8	80·3	56·5	76	53	67	58	57	SW.	SW.	calm	S.	...	...	...	0·80	62
7.	29·860	29·842	29·701	29·20	29·54	29·50	64·9	78·2	59·0	78	58	63·5	56	58	SE.	SW.	SW.	SSW.	...	·03	...	...	60
8.	29·664	30·082	29·628	29·00	29·40	29·43	66·2	74·4	61·0	74	50	65	61	58	SSE.	SW.	SW.	S.	·094	...	·44	...	61
9.	29·738	29·728	29·713	29·12	29·47	29·52	62·8	73·2	56·2	70	49	63	53½	54½	S.	SW.	W.	S.	·044	...	·51	...	60
● 10.	30·008	30·017	29·976	29·39	29·52	29·68	63·7	77·3	53·3	73	51	63	53	60	S.	SW.	SW.	S.	·022	...	·20	...	59
11.	30·016	29·991	29·887	29·35	29·63	29·60	65·4	78·3	58·6	77	59	66·5	59	58	S.	SW.	SW.	S.	...	...	...	...	61
12.	29·852	29·921	29·820	29·14	29·54	29·76	67·4	71·6	62·4	72	50	67	58	55½	.	SW.	SW.	SW.	...	·08	...	...	61
13.	30·116	30·116	30·078	29·46	29·93	30·01	64·3	73·2	56·6	81	49	64·5	61	58	S.	NW.	calm	SW.	·033	...	·10	...	60
14.	30·026	30·006	29·880	29·38	29·88	29·80	67·7	73·6	58·2	71	54	65·5	62	56	E.	S.	calm	SW.	...	·07	...	3·26	54
15.	30·016	30·156	29·976	29·32	29·77	29·92	65·2	70·4	57·9	74	47	63	52	57	SW. var.	W.	calm	SW.	·069	·01	·04	...	61
16.	30·198	30·260	30·166	29·56	30·03	30·07	64·2	69·3	56·3	74	47	63	60	58	W.	SW.	calm	SSW.	...	...	...	...	56
17.	30·100	30·080	29·758	29·52	29·99	29·81	65·4	68·7	53·2	80	61	65	59½	59	E. var.	S.	SW.	SSE.	...	·34	...	...	58
☽ 18.	29·550	29·684	29·485	28·92	29·47	29·34	67·0	72·0	62·3	73	56	66	58	58½	SE.	S.	SE.	SE.	·222	·06	...	...	64
19.	29·636	29·695	29·616	29·02	29·20	29·23	63·4	68·3	58·2	69	59	62	55	57	SW. var.	S.	S.	SSE.	...	...	·15	...	59
20.	29·754	29·825	29·726	29·11	29·41	29·52	63·3	67·7	60·0	76	53	62·5	56½	55	S.	SW.	W.	S.	...	...	...	...	59
21.	29·912	30·006	29·889	29·28	29·50	29·70	63·5	72·0	56·6	70	54	60	63½	57	S.	SW.	SW.	S.	...	·01	...	2·00	58
22.	30·100	30·087	30·039	29·44	29·81	29·85	64·4	72·8	56·9	73	53	65	58	58	SSE.	SW.	W.	S.	...	·20	...	...	59
23.	30·076	30·062	29·926	29·43	29·86	29·72	63·3	69·8	58·0	72	59	63	58	58	S.	S.	calm	S.	·283	·23	...	...	60
24.	29·818	29·802	29·781	29·15	29·60	29·60	63·8	69·3	61·2	69	56	63	57	58	S.	S.	SW.	SSE.	·186	...	·13	...	60
25.	29·800	29·823	29·780	29·16	29·64	29·70	63·0	69·3	58·8	65	50	64	61	58	S.	SW.	calm	S.	...	...	...	...	60
○ 26.	29·794	29·782	29·551	29·21	29·68	29·57	62·3	70·3	55·6	66	52	64	62	58	S.	S.	calm	SW.	...	·70	...	...	58
27.	29·620	29·642	29·575	29·00	29·56	29·66	61·2	67·0	53·5	62	54	58	57	55½	S.	SW.	calm	NE.	·658	·31	·73	...	59
28.	29·854	29·969	29·833	29·27	29·74	29·85	57·7	66·3	55·0	66	51	61	61½	55½	WNW.	W.	calm	NW.	·383	·01	·19	...	57
29.	30·068	30·056	29·688	29·45	29·88	29·78	62·3	65·6	53·2	66	54	62	60	55½	S.	S.	calm	N by S.	...	·40	...	0·75	56
30.	29·804	29·781	29·750	29·22	29·77	29·67	57·7	65·8	54·9	62	55	60·5	60	54	NW.	E.	calm	E.	·322	·21	·86	...	56
31.	29·370	29·718	29·298	28·80	29·48	29·61	57·7	59·6	56·0	67	52	61·5	52	52	S.	SW.	SE.	E.	·261	·26	·96	...	58
Mean.	29·940	29·976	29·850	29·31	29·767	29·773	63·1	69·9	56·7	70·00	53·22	62·6	59·0	57·3					Sum. 2·577	2·92	4·36	6·81	Mean. 58·0

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XXXV. *On the tubular Cavities filled with Gravel and Sand called "Sand-pipes," in the Chalk near Norwich.* By CHARLES LYELL, *Esq., F.R.S., V.P. G.S., &c.**

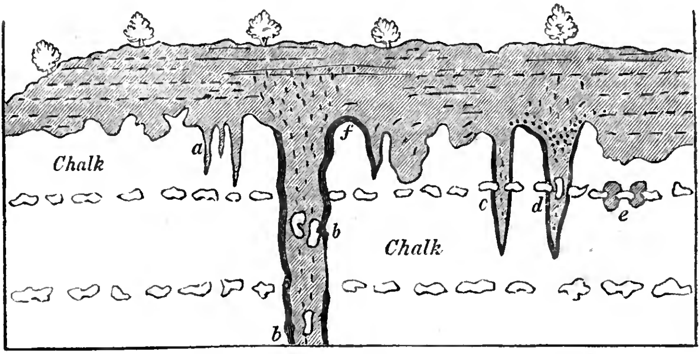
THE white chalk with flints in the neighbourhood of Norwich, is covered with a mass of variable thickness of irony sand and gravel, with some intermixture of red clay, the sand passing occasionally into a ferruginous sandstone. The surface of the chalk when the gravel is removed is extremely uneven, presenting sharp ridges, deep furrows, and pits, and some protuberances which are larger at the summit than the base. In a word, it is impossible to conceive that so soft a rock as chalk could have acquired such an outline simply by ordinary denudation, or could have retained it if once acquired during the accumulation of the mechanical deposit now superimposed. It is equally difficult to refer to any known mode of denudation those deep and narrow hollows, filled with sand and gravel, which are the same as those called in France "puits naturels," and which will form the subject of the present communication.

Form of the Sand-pipes at Eaton.—I was indebted to Mr. Ewing for first calling my attention to some fine examples of these phænomena which he had accurately observed on his property at Eaton, about two miles west of Norwich, where the chalk has been extensively excavated for the manufacture of lime. Cylindrical hollows filled with loose materials, evidently derived from the overlying tertiary formation, are here called "Sand-pipes" by the workmen. They resemble those which occur in many other districts in England, where the chalk is covered by sand and gravel.

* Communicated by the Author.

These deep and narrow pipes are very symmetrical at Eaton, having the form of inverted cones, which at their upper extremity vary in width from a few inches to more than four yards, while at their lower they taper down to a fine point: see fig. 1. The smaller ones, which are usually about a foot

Fig. 1.



Sand-pipes in the Chalk at Eaton near Norwich.

in diameter, seldom penetrate to the depth of more than twelve feet, while the larger are sometimes more than sixty feet deep. They are for the most part perpendicular in their direction, and nearly circular in shape, although they often appear of an oval form when cut through in the precipices surrounding the Eaton quarries, because the plane of intersection is there in reality oblique, and inclined at an angle of about 80° with the horizon. Several sand-pipes often approach very near to each other without any tendency to unite. In proof of this general fact, Mr. Ewing pointed out to me three pipes close to each other, which we explored from top to bottom by digging. The depth of one proved to be twelve feet, that of another nine feet, and that of the pipe placed between the other two six feet. Although they all came within three or four inches of each other, the parting wall of white soft chalk was in no instance broken through; see *a*; fig. 1.

Contents of the Sand-pipes.—The materials filling the sand-pipes are of three kinds: 1st, sand and pebbles; 2ndly, loose unrounded chalk flints; 3rdly, fine ochreous sandy clay, not impervious to water. The rounded pebbles in the first consist chiefly of black flint, while a small number are of white quartz. With these are sometimes seen unrounded fragments of sandstone, with a cement of oxide of iron; the whole agreeing with the contents of the deposit incumbent on the chalk, which at

Eaton is about twenty feet thick. The clay is also similar to the finer portion of that found in the gravel above. As a general rule, admitting of few exceptions, the sand and pebbles occupy the central parts of the pipe, while the sides and bottom are lined with clay. In the clay, at the bottom of one small pipe, which was only six inches wide at the top and a yard deep, I found some black carbonaceous matter, probably derived from the roots of trees which had penetrated from above.

Not a particle of calcareous matter, whether organic or inorganic, occurs in any part of the pipes, either in the middle or at the sides, where the clay is in contact with the chalk. Large unrounded nodules of flint, still preserving their original form and white coating, (*b b*) are dispersed singly, and at various depths, in those larger pipes which exceed one foot or one foot and a half in diameter. The smaller pipes, in which these loose flints never occur, are frequently crossed by horizontal layers of siliceous nodules, as at *c*, *d*, *e*, fig. 1, which still remain *in situ*, not having been removed together with the chalk in which they must have been originally imbedded. Single flints, forming part of these continuous layers, sometimes appear in the middle of a small pipe, as at *d*, fig. 1, surrounded and supported by sand, so that at first sight it is not easy to imagine how it can have retained its position during the substitution of the sand and gravel for the original chalk. But it should be remembered that these flints in the chalk near Norwich, are usually of a large size and irregular shape, and may be still supported at one extremity by the chalky matrix. Neither a loose nodule of flint nor a heap of nodules has ever been observed at the bottom of a sand-pipe at Eaton.

In general there is no order in the arrangement of the materials of the pipes except that the coarse sand and gravel occupies the middle of each and the clay the outside and bottom. There are some exceptions to this rule; but even where coarse sand and gravel come into immediate contact with the chalk they are usually imbedded in a paste of sandy clay, which is wanting in the centre of the pipe. This parting layer of clay, an inch or more in thickness, which lines the walls and attains some thickness at the bottom, may sometimes be traced upwards until it bends round, and continues to intervene between the chalk and overlying gravel, so that the same layer which is perfectly vertical within the pipe becomes horizontal over the chalk, as at *f*, fig. 1. The fine yellow clay at the bottom of some of the pipes has been found by Mr. Colkett, of Norwich, to make a good oil paint of a colour between raw sienna and Roman ochre.

For the distance of several inches, or even in some places four or five feet from its junction with the sand-pipe, the chalk at Eaton is moist and softened, and becomes friable when dried, and is discoloured by containing a slight mixture of fine sand, clay, and iron, the same chalk being quite pure, and perfectly soluble in acids at points more remote from the pipes. In some cracks and interstices of the chalk, even at a distance from the sand-pipes, are found thin leaf-like layers of reddish and greenish clay, which may have been introduced from above through numerous joints which traverse the rock, in a nearly vertical direction, and by which the flints also are sometimes divided. The surface of the flints thus naturally split is discoloured and iron-stained, and distinguishable from that obtained by fresh fracture. At Eaton the joints do not appear to be connected with the sand-pipes, except here and there accidentally. They have in general a more oblique direction than the pipes.

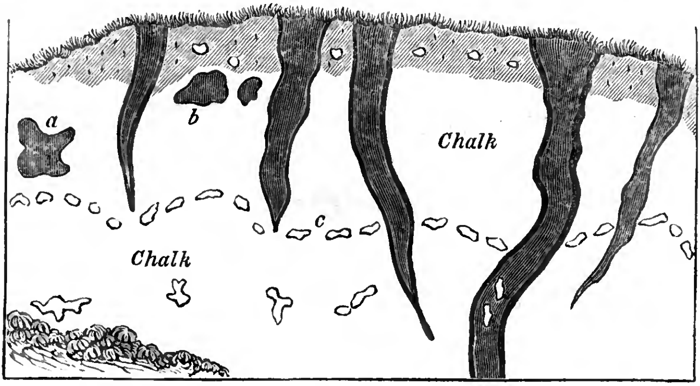
The course of a sand-pipe is usually traceable above the level of the chalk for some distance upwards through the incumbent gravel and sand by the obliteration of all signs of stratification. In some instances however I observed at the mouth or upper extremity of the pipe, as in the pipe *d*, fig. 1, beds of gravel and sand bending downwards, so as to attain a perfectly vertical position within the pipe, precisely as would have happened if horizontal beds had subsided, in consequence of a failure of support from below.

Age of the Gravel.—As to the age of the gravel and sand overlying the chalk at Eaton, there can be no doubt that it belongs to the Norwich crag, as there are not only casts of marine testacea characteristic of that formation in the ferruginous sandstone at Eaton, but also, as I learn from Mr. J. B. Wigham, some shells of the genera Mya, Mactra, Cardium, and Mytilus, in which the calcareous matter is still preserved.

I am also indebted to Mr. Wigham for the following observations. " At Heigham, in the suburbs of Norwich, are sand-pipes resembling those at Eaton, except that they descend in a slanting and often winding course. In the pit represented in fig. 2, which is 30 feet deep, the chalk is barely covered by vegetable soil. Its upper portion to the depth of 4 feet is intermixed with sand and gravel. In the undisturbed chalk below are some irregular cavities, *a*, *b*, from 10 inches to 2 feet in diameter, which have no communication with the surface, and which on examination are found to terminate after penetrating horizontally about 2 feet, the chalk in contact being everywhere solid. They are evi-

dently elbows of tortuous sand-pipes, the other parts of which had been removed during the excavation of the pit. The

Fig. 2.



Tortuous Sand-pipes in the Chalk at Heigham, near Norwich, from a drawing by J. B. Wigham, Esq.

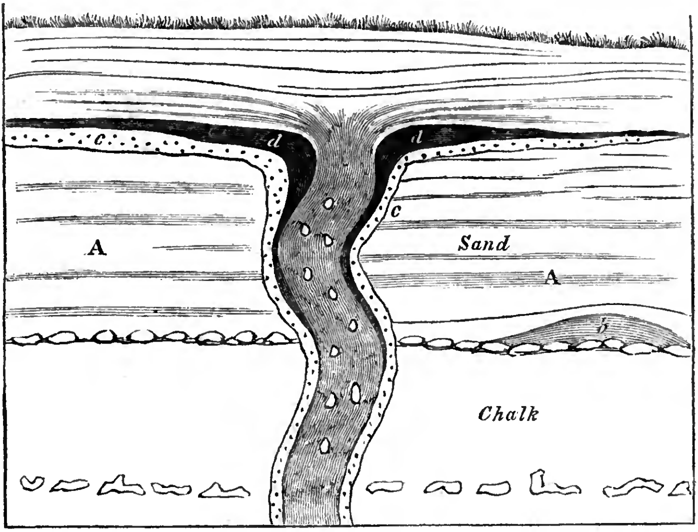
layer of chalk flints, *c*, is far from being horizontal, as will be seen by the sketch, and seem to imply that the chalk has been disturbed.

"At Hellesden, about 3 miles N.W. of Norwich, where similar appearances are exhibited in a chalk pit 20 feet deep, the upper extremity of one pipe, 5 feet in diameter, is covered by undisturbed layers of chalky rubble resembling chalk, alternating with fine clay for a thickness of four feet. One of the pipes in the same pit measures no less than 23 feet across, its depth being unknown.

"At Thorpe is a sand-pipe (see fig. 3.) which is 20 feet in diameter where it enters the chalk. It is filled with gravel, sand, clay, stones, and chalk-flints. It penetrates through 35 feet of chalk, tapering downwards very gradually. It is remarkable for the regularity with which it continues its course through 10 feet of sandy strata, A A, which overlie the chalk, some beds of which, as at *b*, are rich in the shells of the Norwich crag. A layer of light-coloured sandy clay, *c*, fig. 3. (indicated by dots) lines the sides of the pipe for many yards, both where it passes through the chalk and through the overlying arenaceous beds A; this same clay also continuing its course horizontally beyond the opening or upper end of the pipe. The dark bed, *d*, fig. 3. which is in contact with this clay, is an indurated layer of sand coloured by oxide of iron, which contains casts of marine shells, not only where horizontal, but in that part also

which descends into the pipe as far down as where it enters the chalk.

Fig. 3.



Upper Portion of a Sand-pipe at Thorpe, near Norwich, from a drawing by J. B. Wigham, Esq.

"At the junction of the chalk and overlying sand," observes Mr. Wigham, "there occurs at Thorpe, (see fig. 3.) a layer of large flints which have suffered slightly from attrition."

Origin of the Sand-pipes.—We have now to consider in what manner these cylindrical hollows have been first formed and then filled with gravel and sand. If no pipes but those of the smallest size had occurred, we might have imagined that the tap roots of large trees had first pierced the chalk, and then after growing to their full size and decaying had left a vacant space into which loam and gravel fell. But when we reflect on the dimensions of some of the pipes, we at once perceive that more powerful causes must be appealed to.

On consideration of all the facts above described, we can scarcely hesitate to admit the following conclusions: 1st, That the chalk has been removed by the corroding action of water charged with acid, in which the siliceous nodules being insoluble, were left *in situ* in the smaller pipes after the calcareous matrix had been dissolved. 2ndly, It is clear, from the manner in which the large detached flints are dispersed through the contents of the widest sand-pipes, that the excavation and filling of the pipe were gradual and contemporaneous pro-

cesses. For had the tubes, some of which are from 50 to 60 feet deep, and seven yards or more wide, been hollowed out of the chalk before the introduction of any foreign matter from above, great heaps of unrounded flints must have fallen to the bottom, derived from all those intersected layers of flint which formed part of the chalk above. We have seen in the smaller pipes, where the flints are still *in situ,* that the sands and gravel have penetrated many feet, and often yards, below them; so that if these cavities had been further extended in width and depth, the large flints would have been loosened from their matrix, and would have sunk down upon gravel and other matter already introduced, and which had reached a lower level. 3rdly. As a corollary of the above propositions we must hold that the strata of the Norwich crag had been already deposited upon the chalk before the excavation of the sand-pipes, and this is further confirmed by the manner in which the layers of loose gravel of the pipe *d*, fig. 1, and the dark sand with casts of shells, *e*, fig. 3, have sunk into the pipe.

Having then adopted these opinions, and rejected all sudden and violent agency, whether for the erosion or filling of the cavities, it only remains for us to inquire how waters charged with acid may most naturally be conceived to have produced such hollows. If some of the largest pipes of which the bottom has not been yet reached, be prolonged indefinitely downwards and connected with deep fissures, we may suppose that springs charged with carbonic acid rose up at some former period through the chalk and crag while these were still submerged, as we know that in many parts of the bed of the sea such springs do break forth. In proportion as the chalk was corroded, the incumbent substances would subside into the hollow thus formed, and the water would freely percolate the matter thus intruding itself, dissolving any calcareous ingredients which may be associated with it, and still continuing to widen the tube by corroding its walls.

But this hypothesis will not account for the form of the greater number of the sand-pipes, as some, even of those which exceed fifty feet in depth, have been found to diminish gradually downwards to a point. It is therefore more probable that such pipes are due to rain-water, which becoming impregnated with carbonic acid derived from the atmosphere and vegetable soil, has descended into pits or furrows which may have existed on the surface of the chalk. Such water, after dissolving a portion of the chalk, may readily have passed out of the cavities which it gradually eroded, and penetrating downwards might break out again in other places in the form

of springs charged with carbonate of lime, such as are commonly seen to issue from chalk. This hypothesis of the adequacy of pluvial waters was first pointed out to me by Mr. Blackadder of Glammiss, and Mr. De la Beche separately expressed to me the same opinion. But it struck me as an objection to this view, that rain-water would in that case be now in the act of shaping out cylindrical hollows everywhere, both where chalk comes to the surface and where it is overspread by gravel.

But Mr. Strickland, in reply to this objection, has communicated to me in a letter, dated August 31, 1839, the following very interesting remarks. "During a residence of about seven years in the neighbourhood of Henley-on-Thames, I frequently observed subsidences to take place in the gravel above the chalk. They occurred on the top of a chalk hill between 200 and 300 feet above the Thames, and far removed from the action of any running water which might be supposed to have undermined the gravel. The latter formed a stratum from 10 to 20 feet thick above the chalk, and the subsidences appeared to take place quite suddenly, leaving a nearly circular cavity with upright sides from 3 to 6 feet wide, and from 2 to 4 feet deep. As no mechanical action of running water could possibly operate in these situations, it appears to me that the true explanation of the phænomenon must be the corrosive action of acidulated water acting on the surface of the chalk at the particular points to which it may percolate through the incumbent mass of clay and gravel. We have a further evidence of this in the fact, that these subsidences never occur, as far as I am aware, in those places where the chalk is exposed to the day, the rain being there absorbed equally over the whole surface, instead of being conducted to particular points, as is the case where the clay and gravel covers the chalk. If this view be correct, we may infer that many, if not all, of those gravel-filled cavities so common in the chalk, may have resulted from atmospheric agency."

According then to the theory above-stated, we may attribute the larger size of the upper extremity of each sand-pipe to the longer time during which the rain-water has there acted; as the corroding operation proceeded from above downwards, and the percolation for ages of acidulous waters will account for the absence of shells, except as casts, in the contents of the tubes.

In those cases where the tube penetrates the overlying sand and gravel for a certain distance, and then ends abruptly, and is capped by perfectly undisturbed strata, which occasionally consist of chalk rubble, we have only to suppose that the upper portion of the deposit traversed by the tube has been

cut away by denudation, and other beds afterwards superimposed.

As to the sandy clay found at the bottom and round the exterior of the pipes, there can be little doubt, whatever hypothesis we adopt, that this is due to rain-water which in its passage through the gravel and loam has become charged with fine particles of mud and iron, and has parted with these particles at the point where it was absorbed by the surrounding chalk. A very minute quantity of the same mud enfiltered into the contiguous chalk itself, has discoloured the rock and rendered it impure as before described. The moistened state of the chalk for a distance of several feet from each pipe, also shows that this cause is still in operation. The layer of ochreous clay extending upwards beyond the pipe, and intervening between the chalk and overlying gravel or sand, may in like manner be ascribed to the absorption of muddy water by the porous chalk, a vacant space being gradually prepared for the deposition of the mud by the corrosion of the limestone by the acidulous water.

It is scarcely necessary to state that the gradual undermining of the pipes and the successive subsidence of small masses, is an hypothesis which accords well with the fact before alluded to, p. 259, and *f*, fig. 1, that beds of loose gravel and sand once horizontal, now bend into the orifice of some tubes in a vertical direction. Had the entire pipe been filled at once, this arrangement would have been destroyed, and accordingly no such stratification remains in those materials which have descended by repeated movements to considerable depths in large pipes. The grains of the sandstone containing casts of shells which at Thorpe form the dark-coloured bed *d*, fig. 3, which enters the pipe on both sides for many yards, must have been loose and incoherent when they first assumed their present position, and must have been afterwards consolidated within the pipe.

Assuming then that the sand-pipes of Norfolk are due to atmospheric waters, it follows that chalk covered by crag had emerged from the sea before the formation of the pipes. How then shall we explain those cases where chalk not covered by gravel or crag is traversed by large and deep sand-pipes? as at Heigham, fig. 2, and other neighbouring localities. We may answer that aqueous denudation has removed large portions of a deposit once overlying the chalk, and which supplied, in the manner already described, the contents of the sand-pipes. We may also suppose that this same denudation has obliterated all traces of superficial pits and hollows like those above noticed as having been recently formed at Henley.

But if all this be granted, those geologists who have examined Norfolk will admit that the denudation here alluded to must be that which gave to this district its actual valleys, and many of the leading features of its present geographical configuration. We are thus brought round to the conclusion that land in this country must have emerged from the sea after the deposition of the Norwich crag, and yet at a period anterior to that of the denudation just alluded to. But as we know of no denuding agency capable of excavating great valleys in a flat country like Norfolk, except the power of the ocean, operating either at the time of the submergence of land or that of its emergence from the waters, we must infer from all the facts and reasonings above set forth, that land, consisting of chalk covered by crag, was first laid dry before the origin of the sand-pipes, and then submerged again before it was finally raised and brought into its present situation.

For my own part I readily adopt the hypothesis of these oscillations of level, because I have found them indispensable to explain other geological appearances on the coast of Norfolk, not many leagues distant from Norwich, where there is independent evidence of the land having been first laid dry, after the deposition of the crag, so as to support a forest; then submerged again, so as to subside to the depth of 400 feet or more, the signs of the forest being buried under strata several hundred feet thick; and, lastly, of the same tract having been re-elevated, so as to bring the monuments of this remarkable succession of events into view. On this subject I shall shortly enlarge, when treating of the age and origin of "the Mud Cliffs" of Eastern Norfolk.

XXXVI. *On the Use of a Secondary Wire as a Measure of the Relative Tension of Electric Currents.* By JOHN W. DRAPER, M.D., *Professor of Chemistry in the University of New York; late Prof. of Physical Science in Hampden Sydney College, Virginia*.*

[With Figures: Plate I.]

IT is the object of this memoir to establish the following propositions:—

1st. That by means of a secondary wire, we may always determine the relative tension of electric currents.

2nd. That there is reason to doubt whether the processes usually supposed to affect the condition of an *electric current*,

* Communicated by the Author.

are ever attended with any such result; but that when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged.

3rd. That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current: 1st, that tension may be augmented by the sacrifice of quantity; Volta's plan of a reduplicated series, and Henry's ribbon coil in its condition of equilibrium, being examples: 2nd, by the introduction of new affinities in the exciting cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples.

4th. That the law which regulates the connexion of this diminution of quantity, or condensation, with the increase of tension, is the same as that which regulates the analogous phænomena of ponderable elastic fluids.

Incidentally, the examination of certain other points will be entered upon, for example, a brief consideration of Lenz's law of the conducting power of wires; this it will be shown holds not only in the case of Faradian currents, but in the direct currents from hydro-electric and thermo-electric pairs, as has been advanced by some philosophers but denied by others.

The terms tension, intensity, tensile effect, &c. have had very different significations attached to them. From this circumstance a great deal of confusion has arisen, and it is one of the causes of that diversity of opinion and contrariety of theory which obtain in the elementary parts of the science of electricity. For example, Dr. Faraday appears to use the words *tension* and *intensity* as synonyms, expressive, as it were, of elastic force,—chemical authors generally adopting the same signification: "The remoteness from the unexcited state, a condition expressed by the terms *tension* or *intensity*." "By tension or intensity is meant, the energy or effort with which the current is impelled." (Turner, Elem. Chem.)

This confusion of terms leads to a confusion of facts of a much more serious kind. English electricians uniformly state, that the magnetic needle deviating in the neighbourhood of a current, takes no note whatever of the intensity of that current. Continental writers, almost without exception, regard the deviation as a function of the intensity, and the statements therefore appear discordant. Whilst the effect is thus differently described, all agree as to the facts of the case. In what follows, the term *tension* will be used as expressive of the elastic force of the current, that power by which it is en-

abled to pass a resisting medium; the term *intensity* will be strictly confined to the acceptation in which writers on analytical mechanics use it. "By the intensity of a force, we understand its greater or lesser capacity to produce motion," (Boucharlat) and in the case before us, the intensity will be regarded as a function of the quantity and tension conjointly. Thus, the deviation of a magnetic needle does not indicate the tension, but the intensity, of a current.

Suppose now we had a current of electricity passing under a certain tension, along a channel of conduction, as a bar of large dimensions, and were suddenly to interpose in some part of its path a resisting obstacle, as, for example, a slender wire; it is obvious that a certain portion of the current would pass the barrier, a portion determined partly by the character and dimensions of the wire, and partly by the tension or elastic force of the current. Let the wire under all circumstances be the same, the absolute quantity of electricity be constant, but the tension thereof vary. Now, as the tension increases, the quantity that passes the resisting wire will also increase, and as the one diminishes so will the other too. Under these circumstances, the absolute quantity that passes will always be an increasing function of the tension, and as this quantity is under all circumstances measurable by the deviations of the magnetic needle, or by the voltameter, these instruments may be used to determine the tension, by making quantity indirectly the measure thereof.

If, therefore, we send a certain quantity of electricity, as 100 parts, to a resisting wire, and find that of these 50 parts can pass the obstruction, we may assume such a current to have a higher tension than one containing the same absolute quantity, of which only 30 could pass; and to have a much lower tension than one, of which 70, 80, or 90 parts could pass. In all these cases, the amount per cent. of the main current which passes the resisting wire, may be taken as the representative of the tension of that current.

This obstructing, resisting wire, I call a secondary wire.

But it is plain that this amount per cent. of which I am speaking, in introducing this fundamental proposition, is nothing more than the ratio which exists between the quantities passing the large and the little wires respectively. By dividing, therefore, the quantity that passes the secondary wire, by the quantity that passes the large wire, we shall have a numerical representative of the relative tension of the current under consideration.

Let us take an example: a single pair of plates developed

a current of electricity, which when measured at the torsion balance was found equal to 20 degrees; on subjecting this current to a secondary wire, 7 degrees passed it. Its tension might therefore be represented by

·3500.

A second pair was now added in conformity to the first, 31 parts passing; but when subjected to the secondary wire, 18 were indicated. The tension had now become

·5806:

In the same way, by adding three more pairs, the tension rose to ·6346.

It must now be borne in mind, that the numerical determinations thus procured are entirely conventional; their absolute value depends upon the resistance of the secondary wire, and they therefore only express the relative condition of different currents.

As a considerable advantage will be gained, and much repetition avoided, by here indicating the mode adopted for procuring the following measures, I shall describe at once some modifications and additions which are necessary in the torsion balance, the instrument generally employed.

The voltameter has of late come much into use in investigations of this sort, but when compared with the torsion balance, the latter is much more speedy and certain in its indications, and should generally be preferred. In point of fact, the indications of the two instruments are entirely of a different character; the magnetic needle shows the quantity of electricity that is passing in each indivisible portion of time, the voltameter the quantity that has passed at the end of a finite time. In the conditions of the action of the one, time enters as an element, in the other it does not.

By applying a glass thread to the needle, the late Dr. Ritchie greatly improved the accuracy and general utility of the galvanometer; but even with that addition, unless certain precautions are taken, the instrument will not work satisfactorily; the motions of the needle are too versatile, and the tremulous state of vibration into which it may be thrown, are insuperable barriers to accuracy of measurement. A cylindrical trough filled with water is a perfect and admirable remedy for these difficulties.

Another difficulty, which is very generally overlooked, is the excentric position into which the thread is liable to be cast, when the upper micrometer has moved. The construction of the instrument requires, that the axis of motion of the upper micrometer, the axis of the glass thread, the axis of the spindle carrying the needles, and the vane, should be in the

same vertical straight line, through whatever arc the micrometer may have moved. Now it would be very difficult to accomplish this by any system of adjustments.

Whether the instrument is arranged with one or several needles, or whether it has a coil or merely a single strap, the vertical distance from the coil or strap, when the index is brought to zero, ought under no circumstances to vary.

In a climate as hot as that in which the following experiments were made, one of the most unpleasant deviations depends on the thread wrenching in the wax, which is used to fasten it to the needles at one end, and to the micrometer at the other; when the wax softens, and the thread is moved through several degrees, it is not the free part alone that undergoes torsion, but also that which is in the wax, hence arises an error as respects the zero point. This I have always avoided, by ascertaining the zero at the beginning and close of each experiment.

After having had some experience with voltameters, deflecting galvanometers, &c. I am induced to describe the instrument used in these experiments, for it will enable those who are not accustomed to the torsion balance to execute measures very easily, which they might otherwise ineffectually attempt.

AA, BB (Fig. 1. Plate I.) is a glass jar, 16 inches high, open at both ends; at A A it is $2\frac{1}{2}$ inches in diameter, at B B, 6 inches; it rests upon a piece of wood 8 inches by 10. A strap of stout sheet copper, *e f f e*, 1 inch wide and 15 long, is bent into the form indicated; its extremities at *e e* being let into the wood, and bearing mercury boxes, D D. The central part of this strap from *f* to *f*, is placed horizontally, and has a circular aperture and side gap, as is shown in fig. 2. *a a*, through which the spindle carrying the needle can be passed, and works.

The upper extremity of the jar A A, is accommodated with a divided circle, in the centre of which the key G works: this key is ground like a stopcock to a slightly conical figure, it therefore revolves very truly without any shake; it is drilled longitudinally to admit the passage of the glass thread, which is secured in it by means of a perforated straw and a drop of sealing-wax.

The other extremity of the thread enters a little tubular perforation in the ivory axis $n\,n'$, and is also secured therein by wax. Only one needle is used, it is lozenge-shaped and is $4\frac{1}{2}$ inches long. Besides carrying this needle, the ivory axis extends an inch and a half below it, and in a slit at its lower extremity, confines a parallelogram of stout tinfoil, *r r*, an

inch wide and $2\frac{3}{4}$ long. When in use, this vane of tinfoil works in a glass cup, *k k*, $3\frac{1}{2}$ inches in diameter, which is filled with water.

One of the chief improvements in the instrument is connected with the needle, and the axis on which it works. The latter is a small cylinder of ivory; it has two flat faces filed upon it, corresponding to the direction of the needle. On each of these faces, as is represented in fig. 3, is drawn a vertical line, and a little to the right of it are placed five dots. The polar extremities of the needle are accommodated with two upright wires $p\,p'$, $p\,p'$, an inch long, which serve as indexes; and at a distance of 10 or 15 inches, in the magnetic meridian, a plate of metal, not shown in the figure, with a small hole in its centre, is placed to be used as a sight. When an observation is to be made, the experimenter adjusts this sight in front of the instrument, either on its north or south side; and on looking through it, as soon as the needle moves, he sees the index $p\,p'$ traverse *before the scale on the axis.* There is no shake or vibration, even though any one should cross the floor or jar the table, for the index and the scale equally participating in all these disturbances, the motion is almost as steady as that of a shadow on a sun-dial; the vane of tinfoil does not in the least interfere with the accuracy of indication, but effectually stops the oscillations, and the utmost accuracy may be obtained, by previously giving the index $p\,p'$ a slight bend out of the vertical line, and using the five dots as a diagonal vernier.

In the following memoir it will be seen, that the terms primary and secondary wire are occasionally used; the former in a somewhat extended sense. I mean by it not only the thick polar wires that come from the electromotor, those which were used being one-fifth of an inch thick, but include the electromotor itself, no matter what its character may be—if a hydro-arrangement, the plates, exciting liquid, &c. The secondary wires are simply long or slender wires to obstruct the current; of these I have occasionally used two, the first 47 inches long, the second 290: they are of copper, one foot of which weighs 10·65 grs., and are covered with silk.

And lastly, the measures are sometimes arranged in a form such as this,

$$\left.\begin{matrix}100\\50\end{matrix}\right\}\cdot5000$$

in which the large or upper number represents the quantity passing the primary wire, the under or smaller number the quantity passing the secondary wire; and the decimal on the right hand of the bracket, being the quotient of the former numbers, is, as will presently be shown, the representative of the tension.

We have now to examine the foregoing proposition more minutely. Let us call the primary wire, being that which is in connexion with the electromotoric source, A; and the secondary or resisting wire, B. Now how does B act towards currents when they are of variable character? There is no current, no matter how low its tension may be, that will not pass along B to a certain extent: this is abundantly proved by such a wire transmitting a thermal current, of the lowest tension and amount. But at the other extremity of the scale, is there a limiting point? Can a wire conduct electricity of a certain tension, only to a certain amount? I think not, for a wire of small diameter was found upon trial to conduct a thermal current to the extent at one time of 20, and then of 284 parts, the tension in both cases being the same; and if it would do this in the case of currents whose tension is so very low, the same might be looked for in hydro-currents; here, however, when the quantity reaches a certain point, the ignition of the wire ensues, and its physical character is changed. Sir Humphry Davy's experiments lead to the same conclusion, (Phil. Mag. Dec. 1821) nor does there appear to be any limit to the conducting power of a wire, either for high or for low tension. If a wire carries a certain amount of electricity, an increase of quantity or of tension will enable it to carry more, and the converse. To this important point I shall presently return.

As it thus appears that any increase of the quantity which A transmits, involves also an increase of that which passes B, a second question arises, What is the ratio that will be observed in the two cases? If the quantity passing A be doubled, will the quantity passing B be doubled also? This is a very important problem, for if the ratio above-mentioned holds, it would show that an observation by the secondary wire will give the tension independent of the absolute quantity. Let a represent the quantity traversing A, and b the quantity traversing B. Now, if the tension remains constant, and the quantity only is variable, the ratio

$$\frac{b}{a}$$

is always constant, and is entirely independent of the value of a.

This I have endeavoured to prove experimentally. I took a hydro-electric pair of copper and zinc, each of the plates exposing about two square feet of surface, and dipped them to different depths in dilute sulphuric acid. The following table exhibits one of these results.

TABLE A.

Primary Wire.	Secon. Wire.	Calculated.
49	34	34
37	26	25·6
24	17	16·6
13	9·50	9·0

and therefore we infer that the foregoing ratio holds.

Currents of very low tension give proofs of the same fact. A thermal pair of platina and palladium passed 44 through the primary, and 19·50 through the secondary wire; and when by increasing the temperature 236 passed through the primary, 115 went through the secondary wire. In a pair of palladium and silver, 165 and 1130 being passed successively through the primary, 43 and 313 went through the secondary wire. In a pair of iron and platina, 170 and 249 being successively sent through the primary, 79 and 112 respectively passed through the secondary wire.

But let us further suppose, that the quantity of electricity passing at different times through the primary wire A is constant, its tension alone undergoing an increase. If A formerly conducted all that was presented to it, it will under this new condition of things of course still do the same. Such however will not be the case with B, for a greater quantity is now enabled to pass it than before, and the ratio $\frac{b}{a}$ will give a greater value; we shall therefore in this case have a measure of the tension. But if the tension still keeps increasing, b will continually approach to equality with a; and when the tension is infinitely high, these quantities are accurately equal to each other; or in other words, when the elastic force of a current is infinitely high, its tension is unity.

If, on the other hand, the tension becomes lower and lower, b continually decreases, and finally might be found equal to zero. The value of the ratio then becomes zero; and therefore at the two extremes, or where the tension is unity, and where it is zero, the secondary wire, so far from ceasing to act, still truly indicates the condition of the current.

Whilst, therefore, A conducts freely the whole current, B will measure its tension under all circumstances; but in point of practice, we can never make the adjustment here hypothetically indicated, or so arrange a wire A, that it shall conduct *all* the electricity presented to it. Let us therefore here inquire, how this variable condition of *both* wires will affect the result. Let the tension (t), so change by any amount as to

become $(n\,t)$, then a corresponding change will happen in a and b, admitting the principle that the quantities passing through A and B are increasing functions of (t). If then (t) becomes $(n\,t)$, a will become $(n\,a)$. Now if the equation

$$\frac{b}{a} = t$$

holds; $b = a\,t$; but when the change impressed on (t) has happened, b will be equal to the conjoint values of $(n\,a)$ and $(n\,t)$; and if these values be substituted in the former ratio, the result is still equal to $(n\,t)$; so that whatever may be the change impressed on (t) the formula $\dfrac{b}{a} = t$

will always indicate it.

Having thus settled, by the foregoing simple reasoning, the fundamental doctrine of investigation, I next proceed to apply it to the analysis of the different processes, by which a change of tension is supposed to be impressed on an electric current; and this leads to the consideration of the second proposition:—

"That there is reason to doubt, whether the processes usually supposed to affect the condition of an electric current, are ever attended with any such result; but that when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged."

It is popularly supposed, that if we pass an electric current through a wire of certain length, coiled upon itself, a kind of inductive influence will be exerted, so that the current shall become more and more intense as it goes. Or, if two currents are simultaneously passed into a double helix, they will mutually fortify each other.

(*a*.) A wire covered with silk, 48 feet long, and arranged as one circular arc, had a current passed through it, which produced a deviation of 35 degrees. The same wire was then coiled round a piece of wood, so as to make 155 circumvolutions; the deviation was still 35; and therefore no change was impressed on the current.

(*b*.) A thermal current was passed through a straight wire with the following result:—

$$\left.\begin{matrix}42\\22\end{matrix}\right\}\ {\cdot}5238.$$

The wire was then coiled into a helix, the current passed through it, and measured; a powerful bar magnet was next introduced into the helix, and then a rod of soft iron. But in

all these cases the measured numbers were absolutely the same as before. Therefore there is no change impressed on a thermal current, either in relation to quantity or tension, by making it pass along a coiled wire, or by acting on it with a magnet or a bar of soft iron.

(*c.*) The same experiments were made with a hydro-electric current, and they gave the same results.

(*d.*) The above-mentioned (*b*) thermal current was passed along one of the wires of a double helix, and through the other wire a hydro-current was passed, from a single pair of plates; but the tension and quantity remained the same as before. On sending a current of still greater intensity, viz. from a voltaic series of five pair of plates, the same result was still obtained; the hydro-current had power enough to decompose water.

(*e.*) On altering the polar communications, and thereby changing the course of the current, no change whatever in the primary current, either as to quantity or tension, was observed.

It is well known, that by using a long wire as a discharger of a single pair of plates, a spark will be obtained of a much more brilliant character than when the current passes through a shorter wire; it is upon this fact that the flat spiral ribbon coil is constructed. Many electricians have supposed that the results obtained by this beautiful contrivance were partly due to the inducing action of the successive spires, but chiefly to a long and easy conducting channel being open to the current, which gathers momentum in its passage. I have already shown that there is no *permanent* action of induction in the case of a coiled wire,—an observation applying equally to an elongated helix and to a flat spiral. Let us now determine whether the increased tension is due to momentum.

A copper wire, 46 feet long and $\frac{1}{16}$ inch in diameter, being arranged as the discharger of a single pair of plates, a brilliant spark was seen to pass; but with a wire of the same diameter and a foot long, the spark was barely perceptible. The quantity and tension in each case was now determined.

Table B.

Short wire.........	39 12	·3076
Long wire	13 8	·6153

Hence, by the use of a long wire we greatly increase the tension of an electric current. A second experiment, in which a

wire $\frac{1}{8}$, and a third, in which a wire $\frac{1}{5}$ of an inch in diameter, were used, gave analogous results. In neither of these cases, however, did the tension rise so high as in the former; it was lower as the diameter of the wire was greater.

This increase of tension follows the increase of the length of the wire, as the following measures show.

TABLE C.

Ex.		Quantity.	Tension.
1.	Current from a single pair of plates	79	·4177
2.	——————— a long wire introduced	44	·5909
3,	——————— a second ditto, added	27	·7222
4.	——————— third	21	·7619
5.	——————— fourth	15	·8333

Thus, by successively increasing the aggregate length of the discharging wire, the tension continually increased, commencing at ·4177, and finally becoming ·8333. Similar experiments with other wires gave similar results.

Now is this remarkable rise of tension due to a momentum which the current acquires on the wire? Or does it arise from the fact, that the wire acts simply as an obstacle, reacting thereby on the electromotoric plates, the increase of tension being due to them, not it? This is easily determined; for if the rise of tension be due to the plates and not to the wire, a short wire, slender enough to obstruct the current to the same extent, ought to act equally as well as the long wire.

This experiment, the result of which leads to the true theory of voltaic combinations, I shall carefully describe.

I took a copper wire, 46 feet long and $\frac{1}{16}$ inch in diameter, and found that it stopped a certain portion of the current coming from a single pair of plates. The micrometer of the balance was now turned, and the needle brought accurately to zero. Then I cut off from another slender copper wire, such a length (2 feet 10 inches) as to obstruct the current to the same extent as the long wire, the needle being brought when it was interposed in the path of the current to zero. The secondary coil was now introduced; it of course stopped off a certain portion of the current; but the micrometer was again adjusted, until the needle was brought to zero. And now the long wire being introduced, and the slender one taken away, the needle came again to zero. But I suppose, if the long wire had impressed more tension on the current than the slender one, either by momentum or otherwise, more electricity should have passed the secondary wire when it was used, which is not the case.

Again, I took a copper wire, 242 feet long and $\frac{1}{16}$ inch in diameter, and adjusted to it a fine iron wire as before: the extremities of this wire were tinned; it was $12\frac{1}{2}$ inches long. Either of these wires being used as a discharger, brought the needle to the same point of the scale. On using the secondary wire and the long wire together, I adjusted the needle accurately to zero, and then passing the current through the fine wire and secondary wire, it came again to zero. And this was repeated often, and so near was the adjustment, that when an assistant turned first one and then the other wire on, it could not be told which was in action, or whether the current had come along the long or the short wire. A long wire therefore impresses no sort of change on a current, but merely serves as an obstacle; for in the first case we had one wire sixteen times longer than the other, and in this we have a wire more than 230 longer than the one with which it is compared, yet the tension has increased only to the same amount in both.

And the same results were obtained by the voltameter.

The current that flows in a simple closed voltaic circle may be resisted in two ways: 1st, the length of the wire connecting the plates may be increased, as in the foregoing experiments; 2nd, the connecting wire remaining of constant length, the *distance of the plates* may be increased: the result is the same in both cases, a rise of tension.

TABLE D.

Ex.	Distance of the plates in inches.	Quantity.	Tension.
1.	.... 2·75	111	·7297
2.	.... 4·50	50	·7600
3.	.... 9·00	27	·8888

So that, whether we obstruct the current by lengthening the connecting wire, or by increasing the distance of the plates, the general effect is the same, the tension immediately rises; that increase of tension being due to the plates themselves, and not to the channel of conduction. This brings us to the third proposition:—

"That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current. 1st, That tension may be augmented by the sacrifice of quantity; Volta's plan of a reduplicated series, and Henry's ribbon coil in its condition of equilibrium, being examples: 2nd, By the introduction of new affinities in the ex-

citing cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples."

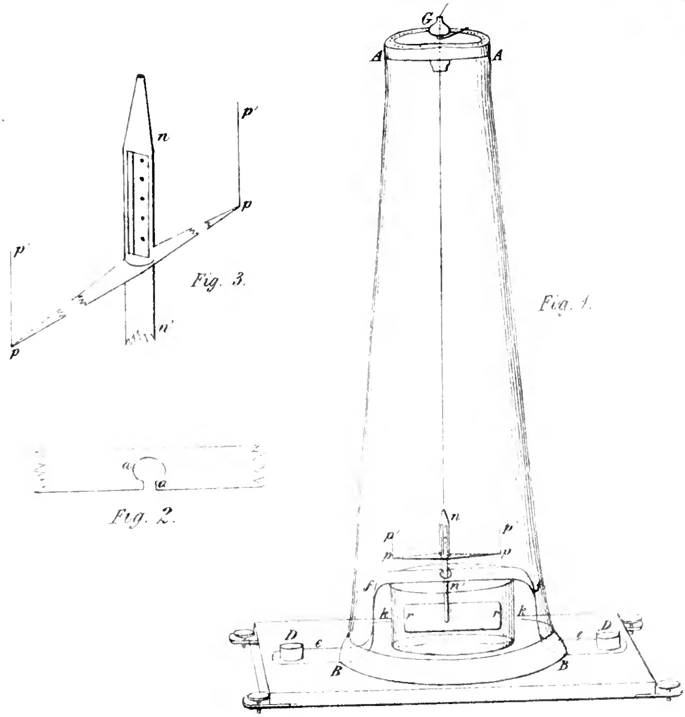
A single pair of plates, under the influence of a long wire, or the spiral coil, presents a remarkable analogy to Volta's pairs arranged in reduplicated series. In point of fact, they may be considered as scarcely differing from each other either in mode of action, or in effect. The study of the single pair under this condition, reveals at once the theory of the voltaic battery.

If we inspect tables B, C, D, we are at once furnished with the fundamental fact which is the basis of explanation. When we compare together the tension and quantity of the electricity flowing in the primary wire, we are struck with the fact, that whenever the one has increased the other has diminished. *No matter what the other conditions may be, whether the communication is made by a long wire or a short one, whether the plates are near or far apart, whenever the quantity is diminished the tension increases, and whenever the quantity increases the tension is diminished.*

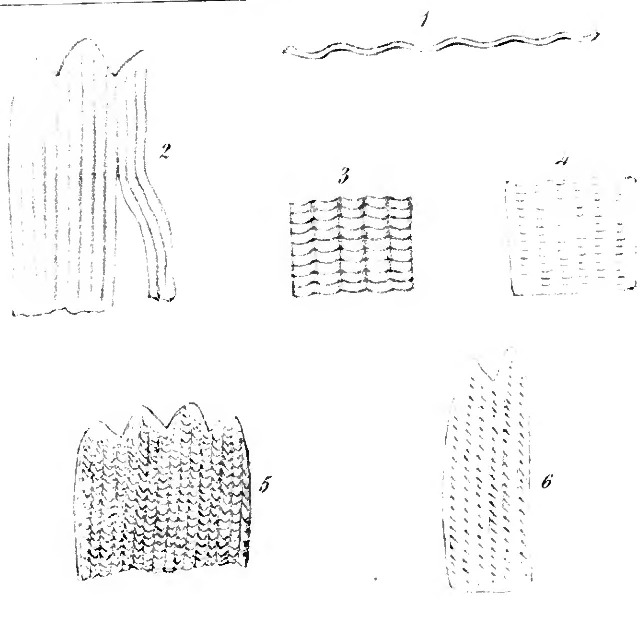
The remarkable analogy of the ponderable elastic fluids, which when their volume is diminished, or in other words condensation takes place, experience an increase of tension or elastic force, is here too broadly indicated to be mistaken.

When I first saw that removing the plates to a greater distance apart determined a given rise in the elastic force of the current, for a time it appeared to me that Dr. Faraday's theory of the tension being due to the affinity of the zinc for oxygen must certainly be incorrect. A more extensive acquaintance with the facts has reversed that opinion. If the tension be determined by the affinity of the metal for oxygen, which must be a constant force, how comes it to pass that moving the plates to a greater distance apart, can cause it to increase? This apparent paradox when properly understood forms a fine illustration of the truth of the doctrine advanced in the 5th, 7th, and 8th series of that philosopher's researches. In what follows I shall therefore regard those doctrines as established.

Let us take a given pair of plates, and connect them together by a slender wire. We find that the quantity that the plates generate is diminished, and its tension is increased; but that this has not happened either by gain of momentum or inductive influence in the channel of communication, and we are compelled to refer the effect to the resistance of the wire, placing the plates and the electrolyte between them in a state of force. If this be the action of a resisting medium, we might suppose that by continually increasing it, we should



Prof. Draper's Improved Torsion Balance.



J. Basire, lith.

Scales of Butterflies Wings, observed by the Rev. E. Craig.

continually increase the tension, and when it became infinitely great, the tension would be so too. But what is the true action of a slender wire, connecting in this way a pair of plates? A certain amount of electricity passes along it, but not the *whole quantity* that the plates could generate in *a given time*: yet we cannot but suppose that *all* that does pass comes from the *whole surface exposed*, and not from a *fractional portion* thereof. The water and zinc are ready to generate, and as it were attempting to drive a fresh quantity of electricity through the wire; and accordingly, as the quantity that actually passes becomes a greater and greater portion of what the system actually tends to put in motion, the tension becomes less and less. The tension would therefore become zero, if the whole circle wires, plates, and electrolyte could carry all that the zinc and water could generate. The limit prescribed to its diminution is the conducting power of the electrolyte, which is the worst conductor of the system.

This hypothetical condition, of a tension ranging near zero, is most nearly approximated to in a thermal pair.

Suppose now that everything remains the same, as respects wires, electrolyte, distance of plates, &c., except that the dimensions of both plates are doubled. Shall we increase the tension? No; for although the surface in action is doubled, and the absolute quantity which the system could generate is doubled, yet the quantity that passes both the primary and secondary wire is also doubled: the ratio $\frac{b}{a}$ is therefore the same as before. For this reason, increasing the magnitude of the plates, increases the quantity only, and not the tension.

Under all these circumstances, therefore, the tension depends on the ratio of the quantity that does pass the combination, to the quantity that the system tends to put in motion.

[To be continued.]

XXXVII. *On the Configuration of the Scales of Butterflies' Wings, as exhibited in the Microscope.* *By the Rev.* Edward Craig, *M.A., F.R.S.E.*

[With Figures: Plate I.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

If the following observations are not rendered unnecessary by earlier correspondents, they are at your service for insertion in the Journal.

There are several notices in the writings of Sir David Brewster, Dr. Goring, and Mr. Pritchard on the lines upon the scales of butterflies' wings which appeared to me to leave the subject still in an unsatisfactory state. They speak of longitudinal lines, of cross striæ, and of two sets of diagonal lines besides, visible on each scale of the *P. Brassica.* It is admitted that the longitudinal and cross lines may be discovered with tolerable facility; but as to the diagonal lines, notwithstanding the distinct averments of Dr. Goring and Mr. Pritchard, a doubt still exists as to their actual existence. Dr. Goring observes that they may be seen as distinctly as the ruled lines in a copy-book, yet admits that there is "an inexplicable mystery about them." Sir David Brewster expresses with some hesitation, in opposition to such authority, his conviction that the diagonal lines are an optical illusion. He states of the whole linear appearances of the scales, that they are none of them lines, but a succession of teeth arranged in lines, and from the great number of lines forming the sides of the teeth they appear dark. Each fibre has teeth on each side of it, and the teeth of one fibre lock into the spaces between the teeth of the adjacent fibres: "the (longitudinal) lines therefore are only *apparent* lines, being composed of a succession of interlocking teeth;" and "the diagonal or oblique lines are optical illusions, from the accidental *alliguements* of the sides of the teeth in different grooves when similarly illuminated by oblique rays." Having been long familiar with these objects, and not being quite satisfied with the above statements, I submitted a great many of these scales to lengthened and intense observation. I examined them through instruments, to the use of which I am very much accustomed, and which, as all microscopic observers are aware, is of great moment for the detection of fallacious appearances: the one is a good achromatic instrument by Chevallier, the other includes a very good Wollaston's doublet, a single garnet lens by Adie of the $\frac{1}{60}$ of an inch focus, and another by Blaikie of the $\frac{1}{30}$. It becomes me, in stating that I have arrived at a different result from these accurate observers, to speak with the greatest diffidence. I shall be thankful if anything stated by me shall serve only as a hint for their further consideration.

The distinct impression upon my mind, after a long series of observations, is that each scale is a film, regularly ribbed or divided by longitudinal fibres which are thicker than the rest of the film, like some corded muslins; and that each thinner portion of the film between the ribs is crossed in a slightly curved direction by still finer fibres, which become

closer in their arrangement as they reach towards the serrated end of the scale. I conceive that the dark longitudinal striæ are not formed by a close combination of interlocked teeth, because I have observed many lacerated scales, which invariably split and tear up the middle of the dark line, and in every such case exhibit a smooth edge without any appearance of teeth. The scales tear down these dark ribs in the same way as some leaves tear easily down the nerves. Moreover it is easy to arrange the lens so as to leave the other parts of the film dark, and the light passing through these longitudinal ribs or striæ in the same way as through the nerves of a leaf. That the cross striæ are thinner than the longitudinal is evident, because when the instrument has been arranged for the most distinct vision of the longitudinal lines, a slight and delicate adjustment of the object *nearer* to the lens throws the longitudinal lines out of the focus and brings out the cross striæ distinctly. Now it is between these two adjustments that the range exists for accurate investigation of the whole phænomena. The cross ribs or striæ may thus be traced running up into the longitudinal, and rising with a curve out of the interstitial furrow.

I would observe also that in consequence of this cross-ribbing of the scale, the longitudinal lines are not the same height all along the scale, but each is liable to irregularities of height at different points, and that this is the cause of the occasionally dotted appearance of the lines for which Dr. Goring blames some microscopes. If the instrument is set so as most distinctly to observe the longitudinal lines, then a slight removal of the object to a *greater* distance will bring the irregular eminences of each line or rib into view.

We are now prepared to observe upon the "mysterious" subject of the diagonal lines. They appear to me to be, as Sir David Brewster states, an optical illusion produced by the arrangement of the cross fibres; a similar effect, on a larger scale, is often observable in printed fabrics for gowns or waistcoats, in which two sets of diagonal lines are seen at a certain distance, but when the pattern is examined closely they are not there. When the scale is removed to a sufficient distance only for showing the cross fibres and not the longitudinal fibres, and the light in the position of the scale is a little oblique, then this delusive appearance takes place. Dr. Goring observes that it was best seen by a lens of moderate power; and certainly, when the magnifying power is only such as not to magnify too much the divisions between the lines, then the delusion of a continued diagonal line is more complete; and it will always appear most complete towards the serrated end

of the scale, because there the cross striæ lie much closer to each other. But if they are examined with more powerful instruments, and the forms adjusted so as to see the rising and sloping portion of the cross striæ, then instead of the diagonal line appearing, the ordinary longitudinal lines will appear "like short hairs or spines in a diagonal direction." In this way they appeared to Mr. Pritchard "through his best instruments." The varieties of appearance seem to me to arise from the instrument commanding at different times the different heights of an uneven surface, and the difference depends upon the distinct vision of the hill, the valley, or the intermediate slope, in a distance which between each longitudinal rib is only the 20,000dth of an inch. The cross striæ are about the 60,000dth of an inch apart, and near the end of the scale still less.

I remain, Gentlemen, yours, &c.

Burton Latimer, Aug. 31, 1839. EDWARD CRAIG.

Description of the Figures. (*Plate* I.)

1. A section of the scales, showing the wavy nature of the ribbed surface.
2. A portion of the scale, showing only the longitudinal lines, and torn along one of the fibres.
3. A portion exhibiting the structure of the film, with the longitudinal and cross striæ.
4. A portion in which the dark longitudinal striæ are thrown out of the focus, and only the cross striæ shown.
5. A portion in which the delusive appearance of the diagonal lines is attempted to be imitated.
6. A portion in which the focus of the lens is so adjusted as to show only a portion of the cross striæ on the rising slope of the furrow.

XXXVIII. *On a new Method of distinguishing Arsenic from Antimony, in cases of suspected poisoning by the former substance.* *By Mr.* J. MARSH.*

IN testing for arsenic in cases of poisoning by that substance, it has always been desirable to render the process as simple as possible, and thereby divest the mind of any ambiguity on the subject. It was with this view that I submitted to the Society of Arts, &c. in the year 1836, my process by hydrogen, a process that I then fondly hoped would have removed all difficulties; but a communication from my friend Mr. Lewis

* Communicated by the Author.

Thompson, and which was inserted in the L. and E. Philosophical Magazine, vol. x. p. 353, has rendered the process in some measure more difficult than was at first supposed, by the discovery of that gentleman, of a compound in which antimony combines with hydrogen to form a gas (antimoniuretted hydrogen). This gas gives off by the process employed, metallic crusts, which much resemble, to the inexperienced eye, the metallic substance obtained from arsenical solutions by the same arrangements. It becomes necessary therefore to find a means of distinguishing these metallic crusts from each other. Many processes which are well known to the experienced chemist may be employed for this purpose; but the misfortune is that all these arrangements suppose a previous chemical acquaintance with the subject; for instance, a good process for this purpose is given by Liebig and Mohr in their valuable Journal (Lieb., *Ann.*, xxiii. 217.) and also a modification of the same, by Berzelius may be seen copied in the Lancet, vol. i. 1838, p. 819, but these are all liable to the above objection, viz. want of simplicity.

I am happy in being able to lay before your readers a very simple distinguishing test for these bodies, and which I have employed in all cases of doubt with perfect success. The means that I use is simply as follows: After the common arrangements have been made for testing for antimony or arsenic, the piece of glass or porcelain on which the metallic crusts are generally received; is to have a single drop of distilled water placed on it; the glass or porcelain is then to be inverted, so that the drop of water is suspended undermost. The gas as it issues from the jet is to be inflamed in the usual manner, but the piece of glass, &c., with its drop of water, is to be held about an inch above the jet, or just above the apex of the cone of flame: the arsenic by this arrangement is oxidized at the same time that hydrogen is undergoing combustion, and coming in contact with the drop of water held above, forms with it a strong or weak solution of arsenical acid, according to the quantity of arsenic present, should that substance have been in the mixture submitted to examination. A very minute drop of Hume's test (the ammoniacal nitrate of silver) being now dropped on the solution so obtained, if arsenic be present, the well-known characteristic lemon yellow colour produced by this test when used for testing for that substance is immediately produced, namely, the insoluble arsenite of oxide of silver. Antimony under these circumstances, from being insoluble, produces no change. I have found it useful, when much arsenic has been present in the matter submitted to examination, to use a clean glass tube, 6 inches long, and about

½ an inch in diameter. I slightly moisten the interior of the tube with distilled water, not allowing the hands or fingers to come in contact with the water: the tube thus prepared is to be held vertically over the apex of the jet of burning gas. By these means a strong solution of the substance is obtained, and which may be tested with perfect ease by Hume's test, or any other of the usual tests employed for arsenic, &c.

I hope that the foregoing process will be found to possess all the delicacy and precision necessary for distinguishing these two bodies from each other, and that it will be the means of removing every doubt from the minds of the experimentalist in future.

Royal Arsenal, Woolwich, June 21, 1839. J. M.

XXXIX. *An Account of a few Independent Notices of America by Middle-age Writers.* By J. O. Halliwell, *Esq., F.R.S., F.S.A., F.R.A.S., &c.**

IN the notes to the new edition of the Travels of Sir John Maundevile, I slightly noticed a very singular passage, which bears evidence of a far higher degree of geographical knowledge than the Englishmen of the fourteenth century have hitherto had credit for. I give it here at length in modern phraseology.

" In that land, nor in many others beyond, no man can see the star Transmontane, which is called the Star of the Sea, or Pole-Star; but men see another star, the contrary to it towards the south, and is called Antarctic. And this is because the world is of round shape, for the part of the firmament that shows in one country, does not show in another country. And men may well prove by experience and subtle trial of intellect, that if a man would search the world and find passage by ships, men might go by ships all about the world, both beneath and above. [*He then gives his astronomical reasons, for which I refer to the work itself.*] By the which I tell you certainly that men may environ all the earth, beneath and above, and return again to his own country, whoever had company and shipping, *and always he would find men, lands, isles, as well as in this country.*"

Maundevile afterwards proceeds to relate a story, which he had heard when young, of a traveller who from India got to Norway, and found several countries in his way. He also adds that there are more than five thousand islands beyond India, which shows that there was a very general notion of the extent of land in the western world.

* Communicated by the Author.

In the work of Ethicus on geography, said to be translated by St. Jerome, and if so of a very early period, reference is made to the existence of large islands in the Atlantic Ocean. It may also be remarked, that there are several tales extant, as early as the fifth and sixth centuries, in which are mentioned several instances of seamen having been driven greatly out of their course from the coast of Spain, and reaching districts never previously visited by Europeans; the Saxons even were accustomed to go to Rome all the way by sea, and it is not unlikely that some of them may have traversed the Atlantic: otherwise how can we account for the general notion which certainly did exist of a country far to the westward beyond the sea, and not coincident with the eastern boundaries of the Old World?

The terrestrial paradise of the middle-ages may possibly have been America; Maundevile, who we must remember was travelling towards the east, describes it as being situated far beyond India. Again, we could hardly expect that the Chinese had not, from a remote period, the knowledge of the existence of a continent in such a relative position to them; and this may account for the tradition current in the East, and a further confirmation of which was not likely to be obtained from that people. From the obstacle thus placed to the free discussion of discovery on the eastern boundary of the Old World, and the difficulty of proving by experience any isolated facts obtained primarily from the vicissitudes of the sea on the western, it is not surprising that we find legend on the one hand and uncertain tradition on the other.

It may be necessary to observe that the preceding notices could have no reference whatever to the discovery of America by the Northmen in the tenth century.

XL. *On the Corollary deduced by Professor* J. Thomson, *from the Proposition demonstrated by him in the Number for July. By* James H. Rigg.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOUR insertion of the following correction of part of a communication which appeared in your last, in an early Number of your Magazine, will oblige yours truly,

July 20, 1839. James H. Rigg.

Professor Thomson gives and demonstrates the following Prop. "Let A B C be a $\triangle$, and through any point P in its plane, let A D, B E, C F be drawn, cutting the sides, or the sides produced in D, E, F: through D, E, F, desc. *a b*

cutting the sides, or the sides produced in D′, E′, F′: join A D′, B E′, C F′: these lines all pass through a common point P′." Hence he deduces the following Cor. "Hence by supposing D and D′, E and E′, F and F′, to coincide, it follows, that the three straight lines drawn from the ∠s, to the points in which the sides touch the inscribed ⊙ pass through the same point; and the same holds regarding the points of contact of any of the ⊙'s touching a side and the continuations of the other two."

Now a little consideration will show that this corollary does not follow from the Prop. By supposing D and D′, E and E′, F and F′ to coincide, the original Prop. becomes this: "If A D, B E, C F meet in a point, then will A D, B E, C F, also meet in a point," which is a mere truism. A D, B E, C F, do, in this case, certainly meet the points of contact of the insc. ⊙, and we know, from other sources, that they meet in a point, but it is not so proved by this proposition.

XLI. *Notice respecting Lantanium. Extracted from a Letter of Professor* Berzelius *to Professor* Kersten *of Freiberg.**

"I HEREWITH transmit to you the oxide of lantanium separated from the protocarbonate of cerium, which you sent me. The oxide of lantanium was discovered in cerite, by Mosander, about the end of last year. Lantanium occurs everywhere with cerium, and [its oxide] has great external resemblance to the oxide of cerium, but is easily separated from it, by dissolving the mixture of both in nitric acid, allowing the solution to evaporate, and exposing the remainder to a red heat. The oxide of cerium becomes by this process almost absolutely insoluble in dilute acids, but the oxide of lantanium dissolves easily, even in the most dilute acids, and may be separated from the solution by carbonate of ammonia, in which it is perfectly insoluble. It has very strong basic properties, and may be dissolved by digestion in solution of muriate of ammonia. Its sulphate, like that of aluminum, is less soluble in hot water than in cold, and is precipitated entirely, while mixed with cerium, by sulphate of potash, but only in part, when it is pure. It is easily distinguished from it by its pale brick-red colour, and separated from it by its ready solution in dilute acids. When heated in hydrogen gas it becomes white, passing into green, without changing its weight, and the salts thus formed have a greenish hue. If heated in air, that is to say, in contact with oxygen, it is of

* Communicated by Prof. Charles Kersten.

a pale brick-red colour, and gives salts of a red amethystine hue. It has also two isomeric conditions distinctly defined. Lantanium is reducible by potassium only from its combination with chlorine. It is apparently a gray, soft, ductile metal, which oxidizes at the expense of water, and gives a hydrate having an alkaline reaction. The reddish oxide changes in water, when hot, to a white hydrate, which after a time turns red litmus paper blue."

Berzelius found the oxide of lantanium in the oxide of cerium which I had separated from monazite, and sent to him for further examination. It occurs with oxide of cerium, alumina, lime, tin, manganese, phosphoric acid, &c.

I have also lately found that it is contained in the *gadolinite* of Ytterby.

Stockholm, May 3, 1839. C. KERSTEN.

XLII. *On a small Voltaic Battery of great energy; some Observations on Voltaic Combinations and forms of Arrangement; and on the Inactivity of a Copper positive Electrode in Nitro-Sulphuric Acid.* By W. R. GROVE, *Esq., M.A., M.R.I.*

To R. Phillips, Esq., F.R.S. &c.

MY DEAR SIR, Wandsworth, Sept. 14, 1839.

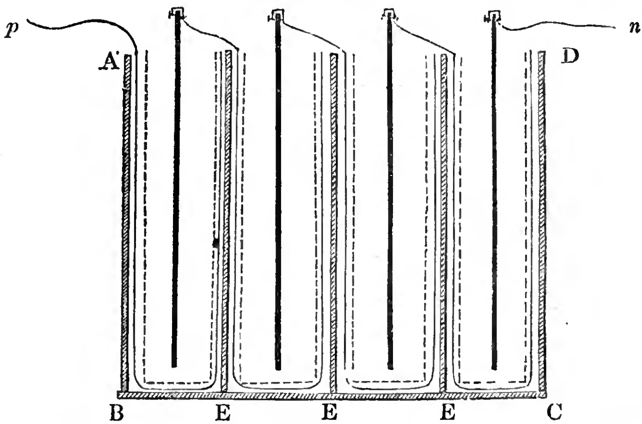
HAVING been requested by many to republish the paper which I read to the Chemical Section at Birmingham, and which has appeared with some inaccuracies in several periodicals, perhaps you will be kind enough to forward the following for publication in the Philosophical Magazine. I remain, my dear Sir, yours faithfully.

W. R. GROVE.

A few days after my paper of the 15th April* was read to the French Academy, I was applied to for directions as to the construction of some batteries on the principle there indicated, and I then suggested some changes of arrangement which I considered advantageous: the apparatus were, however, not completed by the time I left Paris, and I have not since learned how they have succeeded. That which I took with me to Birmingham was hastily constructed, and is capable of much improvement; I will therefore briefly recapitulate its effects, and proceed to describe what appears to me the most convenient method of constructing similar batteries for the usual purposes of experiment. As my trials had been made with a decomposing apparatus much too small,

* Vide Phil. Mag. for May.

I underrated very considerably the power of the battery, which with proper arrangements liberates 6 cubic inches of mixed gases per minute, heats to a bright red 7 inches of platinum wire $\frac{1}{40}$th of an inch diameter, burns with beautiful scintillations steel needles of a similar diameter, and affects proportionally the magnet. The battery consists of four pairs of zinc and platinum foil plates, each metal exposing a surface of fourteen square inches; the whole occupies less space than a cube of 4 inches in the side. I will now describe the mode in which similar ones may be constructed, which will only differ from this in utilizing both sides of the zinc, and of which the accompanying diagram is a vertical section: A, B, C, D, is a trough of stone ware or glass, with partitions E, E, E, dividing it into four acid proof cells.* The dotted lines (I have omitted letters to prevent crowding the figure) represent four porous vessels of a parallelopiped shape, so much narrower than the cells as to allow the liquid which they contain to be double the volume of that which surrounds them; the four dark central lines represent the zinc plates, and the fine lines which



curve under the porous vessels the sheets of platinum foil, which are fixed to the zinc by little clamp screws. The zinc which I generally employ is the common rolled zinc, of about

* Dr. Hare showed that in the common battery insulating partitions could be dispensed with. I have constructed a constant battery of zinc and copper on the same principle; it is inapplicable to that described in the text, as in it each pair is able to decompose water.

As in many districts it may be difficult to obtain a trough of stone-ware, I ought to state that my present one is of wood well lined with cement; the only difference from that above described is that the porous vessels are of less width, so as to contain half as much liquid as the spaces around them, and the position of the metals reversed, the platina being placed within the porous vessels and the zinc without.

the 30th of an inch thick; this is readily amalgamated, and the plates can be so easily renewed that it is not worth while to use stouter. On the zinc side, or into the porous vessels, is poured a solution of either muriatic acid diluted with from 2 to 2½ water, or, if the battery be intended to remain a long time in action, of sulphuric acid diluted with 4 to 5 water*, and on the platinum side concentrated nitro-sulphuric acid (say specific gravity 1·55) formed by previous mixture of equal measures of the two acids. The apparatus should be provided with a cover containing lime to absorb the nitrous gas. I have not given dimensions, as this is a matter of choice: the proportions of the diagram are nearly correct, and taking it at a scale of half an inch to an inch will be found a convenient size, the width to be the same as the depth.

I would now say a few words upon the theory of the battery, assuming as a postulate that the power of voltaic combinations is, *ceteris paribus*, as the resultant of the chemical forces called into action. I cannot explain my views better than by repeating a conclusion which I formerly deduced from experiment (Phil. Mag., Feb. 1839): "It would seem then that the best combination would be one with two metals and two electrolytes, the generating metal being one which has the strongest affinity for the anion of the electrolyte in contact with it, while the other solution is most readily decomposable by its cation, and does not cause a precipitate upon which its own anion would react."

The following then is my mode of explaining the superior energy of this combination compared with those which have been generally employed. In the common zinc and copper battery the resulting power is as the affinity of the anion of the generating electrolyte for zinc minus its affinity for copper; in the common constant battery, it is as the same affinity, plus that of oxygen for hydrogen, minus that of oxygen for copper; in the combination in question, the same order of positive affinities minus that of oxygen for azote †. As nitric acid parts with its oxygen more readily than sulphate of copper, resistance is lessened, and the power correlatively increased. With regard to the second material question, that of cross precipitation: in the common combination zinc is precipitated on

* Dr. Faraday has shown that 1·336 is the best specific gravity for sulphuric acid as an electrolyte. As however the specific gravity of the acid of commerce differs much from that of pure acid, the degree of dilution must depend on the acid employed.

† I have thrown out of the case the resistance to decomposition of the solution in contact with the zinc or generating electrolyte as common to all the three combinations, respect being had to the other conditions; the more easy this is of electrolyzation the better.

the negative metal, and a powerful opposed force created; in the constant combination copper is precipitated, and the opposition is lessened; in this there is no precipitation, and consequently no counteraction. There is still, however, another imaginary voltaic circle (I am tired of iterating the word combination), which would be superior to any of these: it is one of three elements; two metals, or substances having the electrical properties of the metals, and an electrolyte; of these two substances, the positive should be analogous to zinc, but the negative should possess a strong affinity for the cation of the electrolyte, and unite energetically with it, as it separates in a nascent state, or rather should of itself be able to tear it from its associated anion; such a substance is, I think I may say, at present unknown: the nearest illustration I can give is mercury when associated with zinc and a cuperous solution; the peroxide of lead of Professor Schœnbein with zinc and an electrolyte may serve as another. In a circuit of this description we should have actually the sum of affinities instead of their difference, and I can conceive no more powerful hydro-electric arrangement.

To return to what is practicable. In my paper of the 15th of April, I named three combinations, differing only in the solution on the side of the zinc; the three then mentioned were muriatic acid, sulphuric acid, and caustic potash. I have since that tried a variety of solutions on the zinc side, but on the platina side only two new ones, viz. chloric acid and nitro-sulphuric. The first of course was tried merely as a confirmation of the theory, its application being obviously impracticable: the electric effects produced by it differ little from those of nitric acid.

Nitro-sulphuric acid acts as an electrolyte much as nitric acid, i. e. yields oxygen at the anode, and no hydrogen at the cathode; it is consequently equally applicable with nitric acid, and more œconomical. On the side of the zinc I have among others, tried the following, which I give in the order of their superiority. Acids: hydrofluoric, hydrochloric, sulphuric, phosphoric.—Alkalies: potass and soda: Salts, chloride of sodium, nitrate of potass, chlorate of potass, iodide of potassium.—Spring water, rain water, distilled water. Of these the second and third are the only useful ones, as wherever salts are formed the diaphragm is sooner or later disaggregated.

If the operation of the battery be watched, the nitric acid, as we should expect, changes colour, assuming first a yellow, then a green, then a blue colour, and lastly becomes aqueous; after some time, nitrous gas, and ultimately hydrogen, are

evolved from the surface of the platina. The only point worthy of remark is, that the oxidated or dissolved zinc remains entirely, or by far the greater portion, on its own side of the diaphragm: this is an argument for the secondary nature of metallic precipitation by voltaic electricity. And yet this theory of reduction by the nascent hydrogen supposes first, that oxygen quits hydrogen to unite with zinc, and then that it quits zinc to unite with hydrogen: this reversal of affinities is a stumbling-block difficult to surmount. Mr. Daniell has in his last paper given some additional arguments in favour of immediate precipitation; for the present I feel incompetent to pronounce any other opinion upon the subject but that of Sir Roger de Coverley.

As it seems probable that at no very distant period voltaic electricity may become a useful means of locomotion, the arrangement of batteries so as to produce the greatest power in the smallest space, becomes important. I have turned this matter in my mind from reading the letter of Prof. Jacobi, published in the last Number of the Phil. Magazine, and of course I could not avoid measuring the results of my battery with those to which he alludes; my data, however, for such comparison are very imperfect. Prof. Jacobi says, "at present (June 21st) a battery with a decomposing apparatus which will produce from 3 to 4 cubic feet of electrolyzed gas per hour occupies little more space than the page on which I write (stated, I presume by Dr. Faraday, to be 10 by 8 inches) and is about 9 inches high." Now, allowing "the little more space" for the decomposing apparatus, and supposing expense no consideration, I should, as the most convenient way of utilizing the metals, construct four vessels of platina $2\frac{1}{2}$ inches by 9 and 8, each separated from the other by a thin layer of glass and divided internally by two partitions of platina. We should thus have 12 cells $\frac{5}{6}$ of an inch wide, into which would fit the flat porous vessels; and when arranged as a series of four, each pair would expose a surface of platinum of 497 square inches. Supposing then, with Gay-Lussac and Thenard, that for a given combination and given series the chemical power is as the surface, we should have 14 : 6 : : 497 : 213 cubic inches per minute, or nearly $7\frac{1}{2}$ cubic feet per hour. I know this calculation will seem *gigantesque*, and nobody can be more convinced than myself that experiment would fall short of it; still it cannot, I think, be doubted that a very large proportion of this result may be obtained, and by a proper adaptation of funnels and siphons may be kept up for an indefinite period. There is yet a method which would offer a larger negative surface than this; it is to intersect the diaphragms with others, thus

forming a sort of honey-comb of square or hexagonal cells, each containing a tube and zinc rod; but this would be too difficult of manipulation. Another excellent method of œconomizing space, and which is applicable to all these forms, is that proposed by Mr. Spencer of Birmingham, viz. to plait or crimp the negative metal: by this means in a given space the surface may be doubled without increasing the mean distance between the metals*.

The principal difficulty in arrangements of great magnitude would be to prevent the interference of the heating power of the battery. With mine, although my decomposing apparatus contains 8 ounces of liquid, I do not like to continue the decomposition for more than half an hour, as at that time the liquid is much heated. In one case a capsule containing half an ounce of dilute acid was, by the joint effect of gaseous and vaporous evolution, reduced in the space of an hour to a few drops of concentrated acid: the heat was such that the capsule charred to some depth the wood upon which it stood, and water allowed to trickle down the outside, hissed as though from the surface of heated iron.

The electrodes also should bear a certain superficial proportion to the battery or they do not yield the full amount of gas, and are in most cases melted off.

I will conclude this letter with an account of a case of metallic inactivity which I believe (I say it doubtingly) to be new. It is well known that nitro-sulphuric acid acts very sluggishly upon copper, and advantage is taken of this circumstance in the arts, to part silver from copper. If concentrated nitro-sulphuric acid be decomposed by the pile, the negative electrode being of copper and the positive of platina, no particular effect is observable; oxygen is evolved from the platinum, the current passes, and the copper is slightly attacked; but if the positive electrode be of copper, decomposition is entirely arrested, the copper is not attacked and evolves no oxygen: if a galvanometer be included in the circuit, it indicates, after the first instant, only a very feeble current †.

I kept for six hours in nitro-sulphuric acid a very thin strip of copper, which formed the positive electrode of a constant battery; at the end of that period it was nearly divided at the portion just above the surface of the acid. The immersed portion had a thin milky coat, which I believe to have trickled down from the corroded portion, as when this was wiped off,

* In large batteries, the best way would be to zigzag the platina foil round a number of porous vessels; by that means both surfaces of both metals may be rendered available.

† I use the word current in its ordinary sense, without at all intending to attach to it its own somewhat too specific meaning.

the copper was clear and bright, and original delicate file marks could be distinguished on it.

This phænomenon differs materially from that of inactive iron; as the latter metal being made positive electrode, evolves oxygen, i. e. acts as an inoxidable metal, and does not arrest the current.

Admitting the explanation of this latter anomaly, first given by Dr. Faraday (Phil. Mag., 1837), that the iron is by the agency of the electric current closely coated with a protecting film, it would seem that the difference between the case of iron and copper is, that in the former the protecting substance is a conductor, in the latter a non-conductor. The momentary deflexion of the galvanometer is, on this supposition, attributable to the incipient electrolyzation, which occasions the formation of the protecting coat.

XLIII. *On the Geological Position of the Culm- and Plant-bearing Beds of Devon and Cornwall. By the Rev.* D. Williams, *F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I SHALL feel obliged by your inserting the following abstract of the communication I lately made at Birmingham, as it will not only serve as a reply to the article of Mr. Weaver, in your July Number, but I trust effectually set at rest the hitherto disputed position of the culm- and plant-bearing beds of Devon and Cornwall. The two apparent instances of unconformity noticed by Mr. Weaver, and indirectly and reluctantly admitted to be ambiguous, are restored to perfect order and parallelism by an overwhelming accumulation of facts elsewhere, if those instances cited were less equivocal. Working by "*inference*" is a very unsound basis for a broad and bold generalization, and when its results are too confidently enunciated by gentlemen of acknowledged reputation, not only tends to retard the progress of scientific truth, but eventually to damage the reputation of the philosopher for accurate and cautious observation when satisfactorily explained away. Thus at Muddlebridge, where Mr. Weaver "inferred" unconformity, the strike of the Trilobite slates from the east of Bickington, carries them full half a mile to the north of Muddlebridge, the interval being occupied by Coddon Hill grits and slates of a composite and neutral character. Muddlebridge is not only part of a small area of less than half a mile radius, exhibiting great local derangement, but the fact of the Floriferous Beds there dipping N.E., while the Trilobite Slates, a mile and three quarters to the east,

where Mr. Weaver observed them dipping south, proves nothing by inference even; for the latter very commonly dip towards the S.W.; and the Floriferous Beds being almost everywhere arched and inflected, as are the Trilobite Slates along their southern terms, the ascending plane of undulation or inflexion would place them at Muddlebridge in true parallelism with the Trilobite Slates, supposing them to dip S.W. and to have deviated half a mile out of their mean line of bearing. The other instance cited of Rumson Lane S. of Barnstaple, is not only high up among the Passage Beds of No. 8, (and if not within the very confines of the culm-measures certainly in the neutral ground) but altogether so insignificant in dimension, in such a decomposing loose condition, and so concealed by herbage, tangled briar and brushwood, that I should invariably reject such from my field-book, as utterly inconclusive evidence for almost any purpose. Admitting then these instances to be ambiguous, what evidences have we along the north and south borders of the trough to explain them one way or the other? I must here observe, that when I use the term "Coddon Hill grits," I mean a band of rock almost inorganic, but of peculiar and strongly characterized mineral type, which everywhere includes the little trumpery insulatic and elliptical bunches of limestone, which I term the "Possidonia," and Mr. Weaver the "Carboniferous." These grits are admitted on all hands to constitute the base, and to be an integral part of the Floriferous series or culm-measures. Now had Mr. Weaver extended his walk along the coast half a mile north of his Muddlebridge inference, he would have observed, first, these grits alternating with sundry beds of the Floriferous, No. 9, and at length, by a gradual loss of their silica, passing by an insensible transition into the Trilobite Slates; and a more undeniably convincing passage of one great series into another probably cannot be shown on the face of the earth. Nor is this all; tracing the northern confines of these grits, that is, along the line where they approach nearest to the Trilobite Slates, oftentimes having identified them with certainty, we suddenly find their immediate continuation to become as true a Trilobite Slate, with its characteristic fossils, as can be seen anywhere, till eventually at Monbath, on the N. of Bampton, a section of the turnpike road to Watchet, discloses them actually intercalated deep among the Trilobite Slates. If these and countless other gradations and alternations along the north border of the trough, will not satisfy Mr. Weaver of a true conformity, I invite his attention to the results of two years' close and cautious observation along the south border, viz. that from the west flank of Dartmoor to the Atlantic E. and W.,

and from the culm-field to the granite of Caradon Down, N. and S. of the parallel of Launceston, we have constant and repeated alternations, on a great and small scale, of Cornish clay slate or killas, lenticular limestones, carbonaceous slates, Coddon Hill grits, and floriferous shales and sandstones; while on the east of Dartmoor, at Doddiscomb Leigh, five miles north of Chudleigh, we discover the Possidonia limestones underlying a long series of alternations extending to the mouth of the Dart, severally occupying miles of surface on the maps, oftener so minute that they cannot be correctly represented there at all. A cross section of this long line of country exhibits repeated interchanges, at reciprocally varying levels of the Floriferous, Coddon Hill grits, coral limestones, Cornish killas, conformable and unconformable traprocks, and volcanic ash and grit, *each and all*, except the Elvans, characterized by *plants*, *glance flakes of anthracite*, and *carbonaceous matter*; the limestones, with the exception of the included carbonaceous beds and clay slate, being masses of coral architecture, of which, from the many that have been submitted to him, Mr. Lonsdale pronounces that not one is a mountain limestone species. In other localities, both in the slate and limestone, we have chambered and bivalve testacea, almost none of which (except certain ones from the Plymouth limestone, which is higher up in the series) are of carboniferous species; some of the Pethernwin and Landlake fossils being identical with certain ones from the Trilobite slates of Exmoor, others probably belonging to an independent formation not yet determined.

We hope soon, however, to learn more on this head from the critical knowledge of Prof. Phillips, Mr. Sowerby, and Mr. Lonsdale; but I know enough *now* to assert, without doubt or hesitation, that the Possidonia limestones and the plant- and culm-bearing series of Devon and Cornwall, *is a perfectly independent formation far below the carboniferous limestone and its coal-field.* This is all I have ever contended for; and as at length I am left to fight the battle single-handed, and feel my confidence in the stern facts of Devon and Cornwall augment with the falling away of the support which hitherto encouraged me, I fearlessly challenge the banded world of geologists to disprove it.

Bleadon, near Cross, Sept. 9, 1839.

As Mr. Weaver appears to assign no more than a fair value to mineralogical character, and states that " a practical man does not so readily conceive that a glossy clay slate, &c. can be the equivalent of the Old Red Sandstone formation," &c., I would ask him what mineralogical affinity even he could possibly discover in the Coddon Hill grit layers, and the mill-

stone grit and Old Red Sandstone of the English coal-field, which would be common coordinates, if the Possidonia limestones of Devon be the true representatives of the Mountain limestone, the Coddon Hill grits both underlying and overlying the Possidonia limestones? What possible mineralogical analogy did he discover between the floriferous sandstones, and their sometimes vast slaty partings, and the grits and shales of the upper great coal-field? Those slaty partings (which would be the duns and grays of the Bristol coal-field) contain, near Launceston, not only as good roofing-slates as any in Cornwall, but schistose limestones with organic fossils, such as prevail mainly in the grauwacke rocks, while the sandstones (of incalculable thickness) are certainly anything but the Pennant or millstone grit.

In confirmation of what I have previously advanced, within these few days I have discovered the subordinate Coddon Hill grit and floriferous beds constituting an anticlinal axis, throwing off the volcanic ash and clay slate beds or killas on either shoulder. The line of fracture extends from Greeston Bridge, S.E. of Launceston, behind Milton Abbots to Heathfield, N. of Tavistock, where its beds blend with the common mass, showing, as I stated at Birmingham, that the clay slates, &c. of Cornwall are a portion of a great mineral horizon which divides the plant and culm series, No. 9, into an upper and a lower, that great intermediate floor, (which is certainly *above* the Possidonia limestones) containing the red and black slates and limestones of Bampton, Hockworthy, Holcomb Rogus, and Westleigh on the north, and the entire of the coral limestones and volcanic ash and grit beds on the south. Additional evidences are daily offering themselves from all the new country I am traversing, affording such an accumulation of concurrent testimony, radiating from different sources to a common centre, as must satisfy the most cautious and scrupulous judgement of the nether position of the plants and culm of Devon and Cornwall as compared to the mountain limestone and its coal-field. I say again, emphatically and confidently, that either Professor Sedgwick, Mr. Murchison, and Mr. Weaver are in error, when they maintain the contrary, or the evidence derived from superposition and organic fossils is altogether valueless. The question in dispute, however, hardly requires the accumulated testimony that can be adduced. The merest tyro in geology might take his stand on the Chudleigh limestones alone; and pointing to the little horizon of a mile around him and the rocks beneath him, he would defeat the assaults of a thousand adversaries.

I am, Gentlemen, yours, &c.,

Launceston, Sept. 19, 1839. D. WILLIAMS.

XLIV. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 176.]

Island of Punà,

Mouth of the River of Guayaquil.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1824.				
Dec. 8.	79°	7 a.m.	0	
9.	85	3 p.m.		Showers.
10.	77	7 a.m.		Id.
„	84	3 p.m.		
11.	75	7 a.m.		Fine.
„	84	3 p.m.		
12.	77	7 a.m.		Id.
„	85	3 p.m.		
13, 14,	77	7 a.m.		Id.
15.	84	3 p.m.		
16.	79	7 a.m.		Id.
„	84	3 p.m.		
17.	77	7 a.m.		Id.
„	84	3 p.m.		
18.	77	7 a.m.		
19,	77	7 a.m.		Id.
to 25.	87	3 p.m.		
26.	78	7 a.m.		Id.
27.	88	3 p.m.		
28.	80	7 a.m.		Id.
„	89	3 p.m.		
29.	77	7 a.m.		Showers.
„	85	3 p.m.		
30.	75	7 a.m.		
31.	79	3 p.m.		Rain.
1825.				
Jan. 1, 2.	75	7 a.m.		Rain.
3.	83	3 p.m.		
4	77	7 a.m.		Showers.
to 8	85	3 p.m.		
9.	78	7 a.m.		Showers.
„	87	3 p.m.		
10	75	7 a.m.		Showers.
„	85	3 p.m.		

Observations at "El Morro," a village situated about four miles from the coast, at the entrance of the Gulf of Guayaquil. By WILLIAM JAMESON, *Esq.*

1825.	Thermometer. 10 a.m.	Thermometer. 10 p.m.	Hygrometer. 10 a.m.	Hygrometer. 10 p.m.	Remarks.
Feb. 6.	80°·2	80°·3	10°·0	9·5	Clear; much rain the preceding night; evening cloudy and nearly calm.
7.	80·6	80·5	11·5	11·0	Cloudy; rain during the night; evening clear.
8.	85·0	81·5	34·5	22·0	Very clear: evening clear; fine [breeze.
9.	79·3	79·0	10·0	11·0	Cloudy; rain during the night; evening cloudy.
10.	80·0	80·9	14·0	17·0	Rain; evening cloudy.
11.	77·0	77·4	8·0	9·0	Rain, which continued till sunset; evening cloudy.
12.	80·0	80·4	13·5	11·5	Cloudy; evening clear.
13.	83·0	77·0	23·0	14·0	Cloudy; light breeze; evening cloudy with rain.
14.	76·7	79·6	9·0	12·5	Continued rain during the night till 8 a.m.; evening cloudy.
15.	82·5	79·6	13·5	13·0	Clear; wind var.: evening cloudy.
16.	81·0	"	20·0	"	Cloudy; rain during preceding night.
17.	"	"	"	"	
18.	"	78·0	"	11·0	— — ev. cloudy & nearly calm.
19.	81·6	"	19·5	"	Clear and nearly calm.
20.	83·0	78·8	27·5	19·5	Clear, wind S.E.; evening cloudy.
21.	83·2	81·7	39·5	23·0	Cloudy and calm; ground nearly dry; evening cloudy.
22.	80·0	77·2	16·5	12·0	Cloudy; rain during the night; evening cloudy; wind N. with rain.
23.	"	"	"	"	
24.	80·5	"	13·5	"	Cloudy and calm; rain during the [night.
25.	84·7	80·6	32·5	14·0	Very clear; wind SSE.; evening cloudy and nearly calm.
26.	81·0	80·5	19·5	16·5	Cloudy. Thunder and lightning during the preceding night; evening clear and calm.
27.	82·6	81·7	23·0	20·0	Clear. During the preceding night thunder and lightning; evening [clear.
28.	77·0	"	"	"	

THERMOMETER.		HYGROMETER (Leslie's.)	
Mean of 10 a.m.	80°·90	Mean of 10 a.m.	18°·4
10 p.m.	79·63	—— 10 p.m.	14·5
10 and 10	80·26	—— 10 and 10	16·45
Maximum..........	85·0	Maximum	39·5
Minimum	76·7	Minimum	8·0

TABLE *continued.*

1825.	Thermometer. 10 a.m.	Thermometer. 10 p.m.	Hygrometer. 10 a.m.	Hygrometer. 10 p.m.	Remarks.
March.	°	°	°	°	
2.	81·3	81·3	23·0	21·0	Very clear; evening sky overspread with thin clouds; calm.
3.	81·7	81·2	23·0	23·0	Clear; wind variable; evening clear and nearly calm.
4.	82·3	82·1	39·0	37·0	Clear; wind S.E.; evening cloudy and nearly calm.
5.	84·0	78·9	30·0	22·0	Clear; wind variable; evening, wind SE.
6.	,,	,,	,,	,,	
7.	,,	,,	,,	,,	
8.	,,	,,	,,	,,	
9.	82·0	,,	25·5	,,	Clear; wind variable.
10.	,,	81·1	,,	33·5	...... clear and perfectly calm.
11.	85·1	81·7	41·0	27·0	Clear; wind SE.; evening cloudy; wind southerly; light breeze.
12.	83·2	,,	32·0	,,	.... cloudy; wind variable.
13.	,,	82·7	,,	28·0	Cloudy; evening cloudy and nearly calm.
14.	83·7	81·7	34·0	30·0	Cloudy; wind NE.; light breeze; evening cloudy and calm.
15.	83·7	,,	34·0	,,	Cloudy; wind SSE.; light breeze.
16.	,,	80·0	,,	24·0	Clear; wind SE.; evening clear.
17.	,,	82·0	,,	27·5	Clear; wind SSE.; evening calm and cloudy.
18.	84·8	80·6	42·0	23·0	Clear; wind variable; ev. cloudy; wind SE.
19.	84·0	,,	33·0	,,	Clear; wind SE.
20.	,,	,,	,,	,,	
21.	,,	,,	,,	,,	
22.	80·2	,,	27·5	,,	Clear; rain during the preceding night.
23.	82·3	,,	39·0	,,	Clear.
24.	,,	,,	,,	,,	
25.	83·3	81·0	34·5	33·0	Cloudy and nearly calm; evening cloudy; wind SE;
26.	,,	80·5	,,	27·5	Cloudy; wind SE.; evening cloudy and calm.
27.	82·6	80·0	28·0	27·5	Cloudy; wind NE.; evening cloudy; wind SSE.
28.	,,	,,	,,	,,	
29.	,,	,,	,,	,,	
30.	,,	,,	,,	,,	
31.	,,	80·4	,,	29.5	Clear; wind SSE.

THERMOMETER.		HYGROMETER (Leslie's).	
Mean of 10 a.m.	82°·01	Mean of 10 a.m.	32°·2
—— 10 p.m.	80 ·02	—— 10 p.m.	27 ·5
—— 10 and 10	81 ·02	—— 10 and 10	29 ·8
Maximum........	85 ·1	Maximum	42 ·0
Minimum	78 ·9	Minimum	21 ·0

At sunrise the thermometer has been observed this month to indicate 75° 0; and at 1 p.m. 90°·0.

TABLE *continued.*

1825.	Thermometer. 10 a.m.	10 p.m.	Hygrometer. 10 a.m.	10 p.m.	Remarks.
April	°	°	°	°	
1.	,,	80·0	,,	20·5	Clear; wind SSE.
2.	86·4	83·0	45·5	32·0	Very clear; wind variable; evening clear; wind south.
3.	84·0	82·4	36·0	29·5	Cloudy; wind variable; evening clear; wind south.
4.	89·0	,,	53·5	,,	Cloudy; wind variable; at 1 p.m. thermometer 91°·0.
5.	81·7	81·5	20·0	19·0	Cloudy with rain; at 10 clear; evening very clear.
6.	84·0	81·3	33·0	29·0	Cloudy; wind SSE.; even^g cloudy; wind south; very light.
7.	85·0	82·4	35·0	32·0	Clear; wind variable; evening cloudy; wind SE.
8.	78·9	80·2	23·5	19·5	Cloudy; rain during the whole of the preceding night; even^g cloudy
9.	83·2	79·6	24·5	18·5	Clear; wind SE.; evening cloudy; lightning in the N. horizon.
10.	85·7	80·3	36·0	29·0	Cloudy; wind south; even^g cloudy.
11.	82·0	80·4	16·5	14·5	Cloudy; rain during the night; even^g, rain with thund. & lightning.
12.	81·0	80·0	25·5	18·5	Cloudy; wind variable; evening clear; wind south.
13.	83·1	81·0	26·0	24·5	Cloudy; wind variable; evening clear and calm.
14.	82·3	81·4	23·5	29·5	Cloudy; wind variable; evening cloudy and nearly calm.
15.	85·0	81·7	43·5	27·0	Very clear; wind south; evening cloudy and calm.
16.	85·4	81·4	37·5	28·0	Clear; wind variable; evening cloudy and calm.
17.	88·0	81·5	50·5	30·0	Clear; wind SE.; fine breeze; evening cloudy.
18.	84·9	79·3	39·0	26·5	Clear; wind variable; evening very clear and calm.
19.	82·5	80·3	38·5	23·5	Clear; wind variable; evening cloudy with rain.
20.	84·0	79·7	27·5	28·0	Cloudy; wind variable; evening clear.
21.	84·0	79·6	40·0	26·5	Cloudy; wind SE.; evening cloudy.
22.	84·0	81·2	38·0	27·5	Sky overspread with a thin haze and calm; evening cloudy.
23.	83·8	80·6	30·5	25·0	Cloudy; wind variable; evening cloudy; wind SE.
24.	83·7	,,	36·5	,,	Clear; wind variable.
25.	,,	,,	,,	,,	
26.	,,	79·4	,,	35·0	Cloudy; wind SE.; evening sky covered with a thin haze.
27.	88·5	78·7	51·5	28·5	Very clear; wind south; evening cloudy; wind SE.

TABLE *continued.*

1825.	Thermometer. 10 a.m.	Thermometer. 10 p.m.	Hygrometer. 10 a.m.	Hygrometer. 10 p.m.	Remarks.
April 28.	83°·6	79°·3	39°5	23°·5	Cloudy; wind variable; evening clear; wind SE.
29.	84·5	78·7	44·5	28·5	Clear; wind south; evening clear.
30.	84·5	79·0	51·0	29·5	Clear; wind south; evening clear.

THERMOMETER.		HYGROMETER (Leslie's.)	
Mean of 10 a.m.	84°·17	Mean of 10 a.m.	39°.5
——— 10 p.m.	80 ·52	——— 10 p.m.	30 ·1
——— 10 and 10	82 ·34	——— 10 and 10	34 ·8
Maximum..........	89 ·0	Maximum..........	53 ·0
Minimum..........	78 ·7	Minimum..........	14 ·5

Average medium of Thermometer of February, March and April 81°·20.
Do. of Leslie's Hygrometer .... 27 ·01.

City of Guayaquil, about 50 miles from the mouth of the river of the same name. Lat. 2° 12′ S.

Date.	Time.	Thermometer.	Elevation.	Remarks.
1824. Nov. 23.	76°	7 a.m.		Rain.
„	81	3 p.m.		
24, to 28.	} 76	7 a.m.		Showery.
	} 81	3 p.m.		
29.	76	7 a.m.		Id.
30.	80	3 p.m.		
Dec. 1.	76	7 a.m.		Cloudy.
2.	82	3 p.m.		
3.	76	7 a.m.		
„	83	3 p.m.		
11 days 81°·5 max. 76 ·0 min. } Med. 78°·75				
1826. Mar. 11.	79	7 a.m.		Showers.
„	83	2 p.m.		
12.	79	7 a.m.		Id.
„	84	2 p.m.		
13.	79	7 a.m.		Id.
„	85	2 p.m.		
14.	81	7 a.m.		Id.
15.	85	2 p.m.		
16	81	7 a.m.		Id.
to 19.	85	2 p.m.		
20.	79	7 a.m.		Rain.
„	83	2 p.m.		

TABLE *continued*.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1826. Mar. 21.	77°	7 a.m.		Fair.
,,	82	2 p.m.		
22.	79	7 a.m.		Id.
,,	83	2 p.m.		
23.	79	7 a.m.		Id.
24.	84	2 p.m.		
25.	79	7 a.m.		Rain.
,,	85	2 p.m.		
26.	79 5	7 a.m.		Fair.
,,	85	2 p.m.		
27, 28,	79	7 a.m.		Fair.
29.	85	2 p.m.		
19 days. 83°·69 max. 79°·07 min. 81°·38 med.				
April 2.	80	7 a.m.		Fair.
,,	87	2 p.m.		
3.	82	7 a.m.		Rain.
,,	87	2 p.m.		
4, 5,	80	7 a.m.		Id.
6.	85	2 p.m.		
7.	75	6 a.m.		Id.
,,	84	2 p.m.		
10.	77	6 a.m.		Fair.
,,	82	2 p.m.		
11.	82	6 a.m.		Id.
,,	84	2 p.m.		
12.	81	6 a.m.		Cloudy.
,,	84	2 p.m.		
13.	82	6 a.m.		Id.
,,	87	2 p.m.		
14.	82	6 a.m.		Showers.
,,	89	2 p.m.		
15.	82	6 a.m.		Fair.
,,	90	2 p.m.		
16.	80	6 a.m.		Cloudy.
17.	87·5	2 p.m.		
18,	80·5	6 a.m.		Showers.
to 21.	84	2 p.m.		
22.	80	6 a.m.		Id.
,,	82	2 p.m.		
24.	81	6 a.m.		Id.
,,	87	2 p.m.		
25.	77	6 a.m.		Fair.
,,	84	2 p.m.		
21 days 85°·56 max. 80 ·01 min. 83°·28 med.				
51 days. Med. during the Rainy Season 80°·82.				

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1826.	°			
July 8.	70	7 a.m.		Fair and cloudy.
9.	73	2 p.m.		
10,	69	7 a.m.		Id.
to 15.	74	2 p.m.		
16.	69	7 a.m.		Id.
17.	75	2 p.m.		
18.	69	7 a.m.		Clear.
19.	76	2 p.m.		
11 days 74°·5 max. 69 ·25 min. } 71·87 med.				
1829.				
Aug. 13.	71	6½ a.m.		Mornings cloudy with cool breeze: nights clear.
"	72	8½		
"	75·5	12		
"	80	3 p.m.		
"	75	10		
Med. 75°·6.				
14.	70	7 a.m.		
"	76	1 p.m.		
"	78·5	3		
"	77	11		
Med. 76°·5.				
15.	70	7 a.m.		
"	76	12		
"	78	3½ p.m.		
"	72	10		
Med. 74°.				
16.	72	7 a.m.		
"	75	8		
"	77	10		
"	79	1½ p.m.		
"	75	11		
Med. 75°·6.				
17.	73	7 a.m.		
"	76	10		
"	81	3½ p.m.		
"	73	12		
Mean 75°·6.				

Table *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1829. Aug. 18.	70°·5	6 a.m.		Cloudy.
"	72	7½		
"	75·5	10		
"	77	12		
"	79	2½ p.m.		Bright.
"	79·5	3½		123° sun. reflected heat.
"	77	7		
"	73	12		
Mean 75°·43, or of the six days 75°·45.				
Nov. 2.	74	8 a.m.		Very slight showers.
"	79	12		
"	82	2½ p.m.		
"	76	9		
Mean 77° 75.				
3.	73	4 a.m.		Bright.
"	74	8		
"	78	12		
"	82	2½ p.m.		
"	81	6		
"	74	10		
Mean 76°·0.				
4.	73	7 a.m.		Cloudy.
"	74	9		
"	78	3 p.m.		5th at 4½ a.m.; shock of an earthquake.
Mean 75°, or of the three days 76°·25.				
20 days 74° 52 dry season.				
General mean of the year 77°·76.				

[To be continued.]

XLV. *On the Cause of the Holes that occur perforating sheets of Melting Ice. A Prize Essay in the Chemical Class, Marischal College, Aberdeen, Session* 1837–8. *By* John Ferguson, *of Nigg, Student of Medicine**.

THE holes in question are generally about one or two inches, and sometimes more, in diameter. In shape, they are more or less round, but seldom completely cylindrical. In direction, they are most commonly perpendicular to the surface of the ice. After a thaw, they may be readily seen in the sheets of melting ice that float down a river, or on the sea-shore near its mouth. They occur not only in very thin sheets of ice, but in ice upwards of three inches in thickness; and not only in ice disintegrated by melting into a porous texture, but in ice apparently the most compact.

In order to ascertain the cause of these holes, the structure of ice was first considered. Depressions, especially in the under surface of ice, may be observed, and, at such depressions, it may be supposed that the ice will melt into holes; and this in fact happens. But the holes thus formed are seen only in ice less than half an inch in thickness, they are bounded by thin sharp edges, and they are seldom round, so that they do not at all resemble those that are the object of the present inquiry.

Air cells, which are to be found in all ice, varying greatly in their size and frequency in different pieces, but in both respects very equally distributed in the same piece, may also produce small holes in very thin ice; but these are easily distinguished from the larger holes in question, which, as has already been mentioned, occur in ice, although thick, and whether porous or compact.

Ice, in freezing, has a tendency to assume a crystalline structure, which, were it to differ in any one part from the rest, might, in such part, occasion a corresponding facility in the melting. Yet no such difference was detected by inspecting newly frozen ice; and pure newly frozen ice was observed to melt by a warmer atmosphere, or by the sun, without forming any of the holes in question. Ice, indeed, while melting, sometimes appears to consist of a number of prisms laid loosely in apposition, and directed perpendicularly to the plane of the sheet. But I could never succeed in driving any one of these prisms through into a hole, without, at the same time, breaking the surrounding ice. Besides, the holes to be explained are not peculiar to ice of such structure.

* Communicated by Professor Clark of Aberdeen.

As none of the circumstances attending the formation of ice seemed to account for the appearance of the holes in question, attention was next given to ice during the process of melting. After the formation of ice, indeed, and while hard frost continued, nothing rudimental of a hole could ever be observed. Thus the question came to be, What circumstance in the melting gave rise to the holes?

Their frequent occurrence in ice at the mouths of rivers might suggest the idea of their being dependent on salt water —a conjecture supported by the well-known effect of salt in lowering the freezing point of water, and its consequent power of melting ice. The holes, however, occur in inland pools, and in rivers at a distance of several miles from the sea. I froze sea-water by exposing it during a very cold night to radiation—the natural freezing agent. When the ice thus produced was melted, in almost every natural manner, none of the holes were formed, but only a spongy porous texture, in consequence of the rapid melting apart of almost all the ice impregnated with saline substances. I also immersed common ice in sea-water, where the melting took place more rapidly than in fresh of the same temperature; but, in other respects, little, if any, difference of action was observed. From these and similar considerations, I inferred that the formation of the round holes is little dependent on the presence of saline substances in considerable quantity—much less in the exceedingly minute quantities held in solution in ordinary fresh water.

The remarkable property of becoming specifically heavier, as it ascends in temperature from 32° F. to 38° F. belongs to water. On account of this property, water lying in a depression on the surface of a sheet of melting ice, would obviously undergo a circulation, the portion heated by the air descending in the centre, and, when cooled by ice at the bottom, rising by the sides. The immediate consequence of the warmer water being thus always conveyed to the bottom, would be to melt the ice at that part, and thus produce a hole. But when a depression was made in a sheet of ice, and water put into it, the depression, so far as I could observe, continued of the same depth during the whole time of melting. Hence, in such circumstances, the ice probably received the heat as fast as the water.

After having thus far failed to account for the appearance in question, I made an observation that indicated what seems to be the real cause.

In the course of my observations on ice melting on the sides of rivers, I frequently saw holes not entirely perforated

through the ice. In the bottoms of these, I always observed solid bodies—stones, dust, sand, bark of trees, or the like. In one instance, of which I had taken particular notice, I subsequently observed that the imperfect hole had been rendered complete, and corresponded in size to those, the cause of whose formation I had been attempting to discover. I then placed a stone above a piece of ice during sunshine, and examined it on the following day, when a complete hole, bigger than the stone, was found in the ice below the place where it had rested. I confirmed these observations by experiments repeated under varied circumstances, always with like results. Thus, having placed upon ice almost all such small bodies as are usually found on the sides of rivers, I exposed it, in order to melt, both in the rays of the sun and in a warm atmosphere. In all the experiments, holes were produced. I have also seen, where two sheets of melting ice were lying, one above the other, a complete hole through the upper, and an incomplete one through the under sheet, continuing the upper hole, and having at the bottom a bit of clay. Under the same circumstances of temperature, holes were produced when the bodies were put below the ice, so as to allow it to rest on them; and when dust, crumbs of brown wood, and other such light substances, were placed below, they rose up, by capillary attraction, as the holes upwards approached the surface, always keeping the top of it, till the holes were completed. After exposure for one or two days, the holes produced in my experiments, as also those whose formation I had watched, were, by continued melting, ultimately smoothed into the same appearance as those for whose origin I had been seeking to account.

The principle on which these foreign bodies act in producing holes, is their possessing the property of absorbing radiated heat in a higher degree than ice. That this explanation may be applicable to cases in which the body has sunk so far into the ice as to be out of the reach of the direct rays of the sun, or has been originally below the ice, it is necessary that ice should give passage to the heat of the sun's rays. That ice has this property, was determined experimentally. I took a sheet of pretty-transparent newly frozen ice, fully half an inch thick, and having smoothed the inequalities of its surfaces, I found that I could inflame a piece of sealing-wax, by concentrating on it the rays of the sun, by means of a burning-glass, whether the ice was interposed between the sun and the lens, or between the lens and the wax. The effect, indeed, seemed to be little diminished by the interposition of the ice. Yet, that ice does not transmit heat altogether without inter-

ruption, appeared from the possibility of melting a hole in the most transparent ice by the same lens. Hence it appears that heat, radiated from the sun, will pass through ice with little interruption to any opake foreign body, by which it will be absorbed, and by which it will be communicated, by conduction, to the contiguous ice.

The sun is evidently not the only source of the radiated heat absorbable by such foreign body. Thus, we can scarcely suppose that all the sun's heat is intercepted by the clouds, even in a day the most overcast, as such a circumstance never happens with his light; and, when ice is melting in a warm room, not only the fire, but the walls and furniture, are sources of radiant heat. The natural exposure appeared to be most favourable to the formation of the holes; for, in the rays of the sun, a sheet of ice, with a stone above it, was often entirely perforated before the sheet had melted a quarter of its thickness, while, in a warm room, it would first have to melt nearly half its thickness. Perhaps the reason was, in part, the more equal action of heat derived from the atmosphere by conduction, and, partly, the greater absorption by the ice of heat radiated from terrestrial objects, than of heat emanating from the sun.

The explanation of the form of the holes will now be easily understood. As the bodies that produce them, generally either are of themselves round or oval, like most of the small stones found on a river side, or are collected by the washing of the water into circular heaps, as in the case of sand, it is evident that the holes must, from the first, tend to assume a circular form. But, as an additional widening of the hole, during and after its formation, always takes place, owing to the continued melting of the ice, the first irregularities become ultimately less observable. The holes are seldom of equal width throughout, being sometimes wider above than below, at other times the reverse. The foreign body that forms the hole has originally descended, when the hole is wider above—the first formed part of the hole being widened by its longer subjection to the common melting that the whole sheet undergoes—and, in the reverse case, the body has been below the ice, and has ascended.

I have never observed any of the holes, not completely perforating the ice, without having at its bottom some opake foreign body. The holes, during their formation, take a direction perpendicular to the horizon; and the occasional obliquity of the holes, in reference to the surface of the ice, is to be accounted for, by their having been formed when the sheets lay in a position oblique to the horizon.

Before the breaking up of ice by a thaw, the holes most usually are to be observed at the sides of rivers. Indeed, I have scarce ever seen them in the middle. After the breaking up of ice by a thaw, several circumstances serve to increase the number of holes. On the sides of rivers, there is generally a collection of small round stones. Upon these, sheets of ice, when floated up by the rise of the tide, are left, and, during the thaw, soon become perforated with holes upwards. If such holes should not be complete at the time, they are rendered so by subsequent melting. As the stones, in such situations, frequently rise an inch above the surrounding level, they may perforate holes in ice an inch thick. The holes in ice of greater thickness, are commonly produced by bodies lying above the ice, such as the small stones that are washed upon the ice by the agitation of the water, or bits of bark, or of brown wood. Such light bodies, when they float, sometimes produce holes upwards, by being placed below the ice. Holes break out, also, wherever light bodies have been frozen into ice, as occurs in almost every situation. Collections of sand, earth, or clay, constitute another very usual cause of holes.

Standing waters afford peculiar occasions for foreign solids to produce holes in its ice. In a pool, where small single blades of grass had been frozen into ice of two or three inches thickness, I have observed that, during a thaw, the ice immediately surrounding the grass melted into a round hole, often an inch or two wide; and, in ice upwards of three inches thick, where a few withered rushes—one here, one there—were frozen, in a horizontal position, in the middle of the ice, every rush, after a few days' thaw, had a space around it, large enough to hold ten rushes. The vital heat of the grass may have contributed to the melting; but the dead rushes could only have acted like any other opake inorganic solid.

Holes of a similar origin to those in ice, accounted for in this Essay, occur also in melting snow, where dust, or other opake solid, has lain on the surface.

Aberdeen, March 30, 1838.

XLVI. *On the Separation of Lime from Magnesia, and on the Assay of Gold.* *By* Lewis Thompson, *Esq., M.R.C.S.**

To separate Lime from Magnesia.

DISSOLVE the combined earths in dilute nitric or muriatic acid, and precipitate the filtered solution by means

* Communicated by the Author.

of an excess of carbonate of soda; dry the precipitate, and place it in a coated green-glass tube, so disposed that the whole can be heated to a dull red heat; when red hot pass a current of well-washed chlorine through the tube for a few minutes: the lime will be converted into chloride of calcium, but the magnesia remains unacted upon. When the whole is cool, remove the mass from the tube and boil it for a minute or two in water, filter the liquid and wash the insoluble portion (which is magnesia) with water, and precipitate the lime from the mixed liquors by carbonate of soda. The heat should not exceed a dull red, as the mass is apt to become vitrified at the part which touches the tube, and this renders it difficult to remove the contents.

To assay Gold.

Take six grains of the gold to be assayed and place it in a small crucible, with 15 grains of silver, and from 8 to 12 grains of chloride of silver, according to the supposed impurity of the gold; lastly add 50 grains of common salt (chloride of sodium) reduced to a fine powder so as to prevent decrepitation; fuse the whole together for five minutes, and allow it to become cold; then take out the metallic button and beat it into a thin plate, and subject it to the action of dilute nitric acid as in the ordinary mode of parting. By this plan the tedious process of cupellation is avoided, the baser metals being wholly removed by the chlorine of the chloride of silver, and their place supplied by pure silver.

Old Barge-house, July 16, 1839.

XLVII. *On the Use of Barometrical Formulæ for determining the Heights of Mountains.* By S. M. Drach.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It has hitherto been generally supposed that our ignorance of the law of the decrease of heat in the upper regions of the atmosphere has prevented our obtaining an accurate formula for the determination of heights by barometrical observations. I apprehend, however, that this has arisen from an oversight in the analytical calculation; which, if corrected, would eliminate the effect of the diminution of temperature altogether.

In M. Poisson's *Traité de Mécanique* (Art. 624—6.) the following equations are given:

$$p=k\rho\,(1+\alpha\,\theta)\ (1.);\quad dp=-g'\rho\,dz\ (2.);\quad \frac{dp}{p}=\frac{g'}{k}\cdot\frac{dz}{1+\alpha\theta}$$

whereof (2.) = weight of the stratum dz at the height z. Now as (1.) refers to an atmosphere of variable density and temperature, and (2.) to variable density alone, I consider that (2.) (3.) ought to be changed to

$$dp=-g'\rho\,(1+\alpha\,\theta)\,dz\quad (4.)$$

$$\frac{dp}{p}=-\frac{g'}{k}\cdot dz=-\frac{r^2}{(r+z)^2}\cdot\frac{g}{k}dz\qquad (5.)$$

by neglecting the variation of the centrifugal force.

Integrating (5.)

$$-\frac{grz}{k\,(r+z)}=\log.\frac{p}{\omega}=\log.\frac{5550+\mathrm{T}}{5550+\mathrm{T}'}\cdot\frac{h'}{h}-2\log.\left(1+\frac{z}{r}\right)\ (6.)$$

At the lower station $k=k\,(1+\alpha\,t_{\prime})$, $t_{\prime}$=mean heat of lower station; hence as shown by M. P.

$$z=\frac{\overset{\text{metres}}{18337{\cdot}46}\left(1+\frac{3\,t_{\prime}}{800}\right)}{1-{\cdot}002588\cos.2\psi}\left\{\text{conc.}\log.\frac{5550+\mathrm{T}'}{5550+\mathrm{T}}\cdot\frac{h}{h'}\right.$$

$$\left.+\,2\log.\left(1+\frac{z}{r}\right)\right\}\ \left(1+\frac{z}{r}\right)\qquad (7.)$$

Therefore, if A = first factor, $z'=\mathrm{A}'\log.\frac{5550+\mathrm{T}'}{5550+\mathrm{T}}\cdot\frac{h}{h'}$,

$$\delta z=\frac{\mathrm{A}'z'}{r}\left\{0{\cdot}868589+\log.\frac{(5550+\mathrm{T}')\,h}{(5550+\mathrm{T})\,h'}\right\}$$

$z=z'+\delta z$: if the upper station be on a mountain

$$z=z'+\frac{5}{8}\delta z.$$

As an example, the following one, given by M. Poisson, may be taken.

Required the height of Guanaxuato (the true height $=2084^{\mathrm{m}}{\cdot}46$), $h=0^{\mathrm{m}}{\cdot}76315$, $h'=0^{\mathrm{m}}{\cdot}60095$, $\mathrm{T}=t=25^{\circ}3$, $\mathrm{T}'=t'=21^{\circ}{\cdot}3$, $\psi=21^{\circ}$.

M. Poisson finds $z'=2077^{\mathrm{m}}{\cdot}98$; therefore by my formula

$$z'=\frac{2077{\cdot}98\quad\left(1+\frac{3\times 25{\cdot}3}{800}\right)}{1+{\cdot}002(21{\cdot}3+25{\cdot}3)}$$

$$\therefore z'=2081{\cdot}11,\ \frac{5}{8}\delta z=3^{m}{\cdot}90,\ z=2085{\cdot}01,\ \text{error}+0^{m}{\cdot}55=\frac{1}{3800}.$$

M. Poisson's formula gives

$$z'=2077{\cdot}98,\ \frac{5}{8}\delta\, z=3{\cdot}98,\ z=2081{\cdot}96,\ \text{error}-2{\cdot}5=\frac{1}{832}.$$

Supposing the observations to be exact, the only cause of variation is the hygrometrical state of the air, which in the extreme case may cause a difference of $\cdot 0012508=\frac{1}{800}$: and consequently my error shows the atmosphere to be nearly in its mean state, whilst M. Poisson's must suppose the air to be excessively humid.

I am, Gentlemen, your most obedient,

London, June 4, 1839. S. M. Drach.

XLVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xiv. p. 502.]

May 30.—THE reading of a paper entitled, "Fifth letter on Voltaic Combinations; with some account of the effects of a large Constant Battery: addressed to Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain. By John Frederic Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London," was resumed and concluded.

The author, pursuing the train of reasoning detailed in his preceding letters*, enters into the further investigation of the variable conditions in a voltaic combination on which its efficiency depends; and the determination of the proper proportions of its elements for the economical application of its power to useful purposes. He finds that the action of the battery is by no means proportioned to the surfaces of the conducting hemispheres, but approximates to the simple ratio of their diameters; and hence concludes that the circulating force of both simple and compound voltaic circuits increases with the surface of the conducting plates surrounding the active centres. On these principles he constructed a constant battery consisting of seventy cells in a single series, which gave, between charcoal points separated to a distance of three-quarters of an inch, a flame of considerable volume, forming a continuous arch, and emitting radiant heat and light of the greatest intensity. The latter, indeed, proved highly injurious to the eyes of the spectators, in which, although they were protected by grey glasses of double

* Abstracts of Prof. Daniell's preceding letters have appeared in Lond. and Edinb. Phil. Mag., vol. viii. p. 421; vol. ix. p. 376; and vol. xii. p. 364.

thickness, a state of very active inflammation was induced. The whole of the face of the author became scorched and inflamed, as if it had been exposed for many hours to a bright midsummer's sun. The rays, when reflected from an imperfect parabolic metallic mirror in a lantern, and collected into a focus by a glass lens, readily burned a hole in a paper at a distance of many feet from their source. The heat was quite intolerable to the hand held near the lantern. Paper steeped in nitrate of silver and afterwards dried, was speedily turned brown by this light: and when a piece of fine wire-gauze was held before it, the pattern of the latter appeared in white lines, corresponding to the parts which it protected. The phenomenon of the transfer of the charcoal from one electrode to the other, first observed by Dr. Hare*, was abundantly apparent; taking place from the *zincode* (or positive pole,) to the *platinode*, (or negative pole). The arch of flame between the electrodes was attracted or repelled by the poles of a magnet, according as the one or the other pole was held above or below it: and the repulsion was at times so great as to extinguish the flame. When the flame was drawn from the pole of the magnet itself, included in the circuit, it rotated in a beautiful manner.

The heating power of this battery was so great as to fuse, with the utmost readiness, a bar of platinum, one-eighth of an inch square: and the most infusible metals, such as pure rhodium, iridium, titanium, the native alloy of iridium and osmium, and the native ore of platinum, placed in a cavity scooped out of hard carbon, freely melted in considerable quantities.

In conclusion, the author briefly describes the results of some experiments on the evolution of the mixed gases from water in a confined space, and consequently under high pressure; with a view to ascertain, first, in what manner conduction would be carried on, supposing that the tube in which the electrodes were introduced were quite filled with the electrolyte, and there were no space for the accumulation of the gases; secondly, whether, decomposition having been effected, recombination would take place at any given pressure; and lastly, whether any reaction on the current-force of the battery would arise from the additional mechanical force which it would have to overcome. These experiments he purposes pursuing at some future time.

A paper was also read, entitled, "An experimental inquiry into the influence of nitrogen in promoting vegetable decomposition, and the connexion of this process with the growth of plants." By Robert Rigg, Esq. Communicated by the Rev. J. B. Reade, A.M., F.R.S.

The author considers it as a general fact, to which there are very few if any exceptions, that vegetable bodies in the state in which they are produced in nature, undergo spontaneous decomposition when kept under circumstances favouring such an action; and that, from the decomposition of each, compound products peculiar to that

* This phenomenon, we believe, was first observed by Prof. Silliman; see Phil. Mag. First Series, vol. lxiii. p. 243; lxv. p. 283.—Edit.

substance result. A variety of experiments are detailed and tabulated; the first series of which contains those made on solutions of compounds, such as sugar, honey and extract of malt, showing that in each the amount of spontaneous decomposition is in proportion to the quantity of nitrogen it contains. This law is found to extend to those parts of plants which are not in solution in water, but which remain in their natural state of elaboration, only having their texture broken down.

The author is led to infer from his experiments that the chemical action to which any vegetable matter is naturally disposed, may, to a certain extent, be changed into some other, differing both in its kind and in its products; and that in order to effect such a change nothing more is required than to excite in other vegetable matter mixed with the former, some action which shall preponderate over the rest, so that the whole mass may obey this new and predominant influence. The vapour which is disengaged during the rapid decomposition of vegetable matter he finds to be highly noxious; and thence draws the inference that the Author of the universe has wisely ordained, that, when young plants, containing large quantities of nitrogen, are by any means checked in their growth, they shall be consumed by certain insects; which insects may be conceived to form one of the links of that harmonious chain which binds together all the parts of the universe.

The relation between the decomposition of vegetable matter and the growth of plants is apparent from the similarity of the influence of nitrogen on both these processes: this double function which nitrogen performs in favouring chemical decomposition by the roots of plants at the same time that it assimilates the matter thus formed in their other parts, is regarded by the author as another link in the same chain. In support of this view, he adduces the different chemical constitutions of the roots of the same plants when very young, and when fully grown. He finds that when plants have to perform the important offices of providing nourishment for the rapid growth of their young and tender shoots, they contain a quantity of nitrogen two or three times greater than that which they possess when fully grown; and he concludes by showing that, in accordance with these views, the seeds, roots and plants when placed in highly decomposing or decomposed matter, cease to grow, and under these circumstances, their germinating or vegetating power being superseded by the chemical action established in the matter which surrounds them, the whole becomes one mass of contaminated and infectious matter.

June 6.—A paper was read, entitled, "Experiments on the chemical constitution of several bodies which undergo the vinous fermentation, and on certain results of that chemical action." By Robert Rigg, Esq., F.R.S.

The special object of this paper is to show, first, that sugar is not constituted of carbon and water only; secondly, that during the vinous fermentation water is decomposed; thirdly, that neither pure carbonic acid nor alcohol is, in the common acceptation of the term, the product of this chemical action; and fourthly, that fermented

liquors owe some of their valuable qualities to peculiar products formed during fermentation.

In order to trace the various chemical changes which occur in this part of his research, the author has had recourse to numerous experiments, the details of which are recorded in tabular forms. The first table exhibits the analysis of different kinds of sugar, honey, treacle, grape-juice and extract of malt and hops, the general result of which is that all these compounds contain oxygen in excess above the proportion in which it exists in water, and that they also contain a small quantity of nitrogen. He shows, by two independent modes of experimenting, that these bodies, when in solution, cannot be the only compounds undergoing decomposition during that fermentation, which has for its product spirit and carbonic acid; and in proof of this proposition he recapitulates the different elements in the compounds at the commencement and at the conclusion of the experiments. He finds that when the proximate elements are made the subject of calculation, the weight of the alcohol (constituted of two equivalents of carbon, three of hydrogen and one of oxygen) added to that of the carbonic acid and undecomposed sugar, exceeds the weight of the sugar employed by about 7 per cent. On recapitulating the ultimate elements, he finds that the hydrogen and the oxygen in the compounds after the fermentation exceed their quantity in the sugar experimented upon, by 15 per cent. of the former, and nearly 14 per cent. of the latter; and as a proof that no material error is occasioned by the mode of experimenting, it is found that the difference between the quantity of carbon at the first and at the last is very small.

Having arrived at these conclusions, the author infers from his experiments that the water of solution is decomposed by the carbon of the dissolved vegetable matter, in every case of the vinous fermentation, and in proportions proximately represented by the following formulæ: viz.

$$\left.\begin{array}{lll} \text{2 equivalents of the carbon of sugar} \ldots\ldots & 6\cdot12 \ \ldots\ldots & 12\cdot24 \\ \text{2 equivalents of the hydrogen of the water} & 1\cdot \ \ \ \ \ \ldots\ldots & 2\cdot \end{array}\right\} 14\cdot24 \text{ olefiant gas.}$$

$$\left.\begin{array}{lll} \text{1 equivalent of the carbon of the sugar} \ldots & 6\cdot12 \ \ldots\ldots & 6\cdot12 \\ \text{2 equivalents of the oxygen of the water} \ldots & 8\cdot \ \ \ \ \ \ldots\ldots & 16\cdot \end{array}\right\} 22\cdot12 \text{ carbonic acid.}$$

This decomposition he conceives is brought about by the influence of nitrogen, a very small quantity of which enters into the constitution of the olefiant gas, forming the base of all spirituous fluids of the alcohol and ethereal kind; and thus each compound experimented upon, forms these products in proportion to the quantity of carbon which undergoes chemical change, whether that compound be sugar, soluble parts of malt, grape-juice, or any other body.

The author proceeds to inquire into the constitution of the products which result from this chemical action. He finds the gas which is given off to be composed of carbonic acid, mixed with a small proportion of carburetted hydrogen; and the spirit, when subjected to careful analysis, to resolve itself into carbonic acid, water, and a very small proportion of nitrogen: and in further proof of the existence of this last-mentioned element in spirit, the author

has recourse to the ultimate analysis of the charcoal obtained from alcohol and olefiant gas, a mode of experimenting which he conceives is less liable to error.

He states that the views thus sought to be established are confirmed, not merely by these direct experiments, but likewise by other changes which fermented liquors undergo on being kept under circumstances favourable for further chemical action; and that having proved the existence of such a compound as one constituted of carbon, hydrogen and nitrogen, and shown that water is decomposed during its formation, he thinks we are enabled to account for many other changes which occur during the decomposition of vegetable matter and the growth of plants: whence he proceeds to show that evidence of the presence of such a compound as the above in fermented liquors is afforded by the changes which take place in consequence of keeping them.

On subjecting to ultimate analysis the semi-fluids obtained by exposing fermented liquors to a temperature of 110° Fahr., the author found a decided difference between the chemical constitution of those procured from old, and those from new vinous fluids; and on searching for the law by which these differences are regulated, he found that the spirituous part entered into such combination with the excess of oxygen before referred to, and the undecomposed vegetable matter, so as to form with them new compounds.

After experiencing some difficulty in effecting a separation of these newly formed bodies, he found that while rectified pyroxylic spirit, of the specific gravity from 830 to 835, scarcely dissolved an appreciable quantity of the extract of malt and hops, considerable portions of the semi-fluids obtained from old ale and old porter were, by agitating, dissolved therein. By using this spirit as a solvent he effected such a separation as enabled him to discover decided differences between the proximate chemical constitution of the semi-fluids obtained from old ale and old porter, and those from liquors of the same description when new.

In accounting for many of the phenomena which accompany the vinous fermentation, the author observes that the small excess of oxygen found in all these compounds, which undergo this chemical action, is an essential and indispensable circumstance; a conclusion which is corroborated both by the formation of these new compounds which have been described, and by the generation of the acetic, tartaric, or malic acid, which is found in fermented liquors at all times, and in quantities varying according to the situations under which these fluids have been kept.

June 13.—The following papers were read, viz.:—

"Researches on the Tides. Tenth Series. On the Laws of Low Water at the Port of Plymouth, and on the permanency of mean water." By the Rev. W. Whewell, B.D., F.R.S., Fellow of Trinity College, Cambridge*.

* The former series of Prof. Whewell's Researches on the Tides have already been noticed in our pages. See Lond. and Edinb. Phil. Mag., vol. xii. p. 54 note; and vol. xiii. p. 457.

In this memoir the author investigates the question, how far the *mean water*, that is the height of the tide midway between high and low water, is permanent during the changes which high and low water undergo. That it is so approximately at Plymouth having been already ascertained by short series of observations, it was desirable to determine the real amount of this permanency by induction from longer series of observations. A period of six years was chosen for that purpose; and the method of discussing these observations was the same, with slight modifications, as in former researches.

The height of low water, cleared from the effects of lunar parallax, and very nearly so from those of lunar declination, and compared with the height of high water, similarly cleared, enabled the author to ascertain whether the mean water also was affected by the semi-menstrual inequality. The results of the calculation show that the height of mean water is, within two or three inches, constant from year to year; and that, for each fortnight, it has a semi-menstrual inequality amounting to six or seven inches;—the height being greatest when the transit is at 6h. and least when at 11 h.,—the immediate cause of this inequality being, that the semi-menstrual inequality of low water is greater than that of high water: this inequality, however, is probably modified by local circumstances.

These researches have also verified the theoretical deduction, that the height both of low and of high water being affected by the moon's declination, their mean height partakes of the variations in this latter element, in successive years, consequent on the change of position of the moon's orbit. At Plymouth the increase in mean low water amounts to about two inches for each degree of increase in the declination. In the high water this change is less marked.

The parallax correction of the height of low water is obtained from all years alike, by taking the residue of each observation, which remains when the semi-menstrual inequality is taken away, and arranging these residues, for each hour of transit, according to the parallax. The declination correction is obtained in a manner analogous to the parallax correction, from each year's observations, with some correction for the variation in the mean declination of the moon in each year.

"Researches on the Tides. Eleventh Series. On certain Tide Observations made in the Indian Seas." By the Rev. W. Whewell, B.D., F.R.S., Fellow of Trinity College, Cambridge.

This paper contains the results of the examination by the author of certain series of tide observations made at several places in the Indian Seas, which were forwarded to the Admiralty by the Hon. East India Company. These localities were Cochin, Corringa River, Surat roads in the Gulf of Cambay, Gogah, on the opposite side of the same gulf, and Bassadore, in the Island of Kissmis in the Persian Gulf.

"On the Electrolysis of Secondary Compounds." In a letter addressed to Michael Faraday, Esq., D.C.L., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain, &c.,

&c. By John Frederic Daniell, Esq., F.R.S., Professor of Chemistry in King's College, London*.

The discovery of definite electro-chemical action naturally suggests the inquiry into the relative proportion of that part of a voltaic current, which, in the case of its decomposing a saline solution, is carried by the elements of the water, and that part which is carried by the elements of the saline compound, and into the definite relations, if any such there be, subsisting between the two electrolytes so decomposed. This question was the origin of the investigation which forms the subject of the present letter. The power which the author employed in this experimental inquiry was that of a small constant battery of thirty cells, six inches in height, with tubes of earthenware, charged in the manner he has described in his former communications to the Society. The result of the first experiment evidently indicated that the decomposition of one equivalent of water was accompanied by the decomposition of an exact equivalent of sulphate of soda. The author then endeavours to ascertain whether the power of the current is equally divided between what had hitherto been regarded as the two equivalent electrolytes. The first experiments he made in order to determine this point seemed to lead to the extraordinary conclusion, that the same current which is just sufficient to separate an equivalent of oxygen from an equivalent of hydrogen in one vessel, will at the same time separate an equivalent of oxygen from one of hydrogen, and also an equivalent of sulphuric acid from one of soda in another vessel.

The author then examines the remarkable phenomena relative to the transfer of matter from one electrode to the other without the decomposition of the transported compound; a phenomenon which was first observed by Mr. Porret in glass cells divided into two compartments by a diaphragm of bladder.

Having observed that the products of electrolyzation cannot be kept long separate in their respective cells, on account of the ultimate mixture of the liquids on the platinode side of the diaphragm, the author was led to construct an apparatus by which this evil is remedied much more perfectly, and to which he gives the name of *the double diaphragm cell.* It consists of two cells, formed of two glass cylinders, with collars at their lower ends, fitted by grinding to a stout glass tube bent into the form of the letter U, and firmly fixed on a wooden post. The current transmitted by this double cell is more retarded than when passing through the single cell, on account of the greater distance of the electrodes; but it answers its intended purpose of stopping the transfer of the liquid even in the case of saline solutions, and there is still sufficient conducting power to render it powerfully effective. Experiments were then made to ascertain whether in the electrolysis of the dilute sulphuric acid any transfer of the acid takes place; from which the author concludes that during the electrolysis of an equivalent of water, a portion of acid passes over from the platinode to the zincode; and possibly an

* See p. 312, *ante*.

equal portion of water also passes over from the zincode to the platinode. These experiments appear to confirm the results previously obtained; namely, that one fourth of an equivalent of sulphuric acid passes from the platinode to the zincode for every single equivalent of a compound which has been electrolyzed by the current.

The author then proceeds to examine the following question, viz.: does the acid during its transfer, in the case of the mixed acid and water, or do the acid and the alkali, in the case of the saline solution, convey any portion of the current which effects the simultaneous decomposition of the water in both instances? He next investigates the action of the voltaic current on the aqueous solution of the chlorides, as the simpler constitution of this class of salts promised to throw some light on the nature of the electrolysis of secondary compounds.

The results of all these experiments tend to the establishment of the fundamental principle, that the force which is measured by its definite action at any one point of a circuit cannot perform more than an equivalent proportion of work at any other point of the same circuit; and that the current, which is measured by its electrolysis of an equivalent of simple chloride of lead, cannot at the same time be sufficient to electrolyze an equivalent of chloride of sodium, and an equivalent of water, at the same electrodes. The sum of the forces which held together any number of *ions*, in a compound electrolyte, could, moreover, only have been equal to the force which held together the elements of a simple electrolyte, electrolyzed at the same moment in one circuit.

In applying these principles to the electrolysis of the solution of sulphate of soda, water seems to be electrolyzed, and at the same time the acid and alkali of the salt appear in equivalent proportions with the oxygen and hydrogen, at their respective electrodes. It cannot be admitted, that after the decomposition of the water there is any excess of force applicable to the decomposition of the salt; but it must be concluded that the only electrolyte which yields is the sulphate of soda, the *ions* of which, however, are not the acid and alkali of the salt, but an *anion*, composed of an equivalent of sulphur, and four equivalents of oxygen and the metallic *cathion*, sodium. From the former, sulphuric acid is formed, at the *anode*, by secondary action, and the evolution of one equivalent of oxygen; and from the latter, soda at the cathode, by the secondary action of the metal, and the evolution of an equivalent of hydrogen. The formation of these secondary electrolytes, and compound anions and cathions, will probably furnish the key to the explanation of many of those decompositions and recompositions, to which the presence of water is necessary, such as those of nitric acid on the metals, and the formation of Schœnbein's circuit*: but the author reserves for a future opportunity the examination of this hypothesis, as well as of the general question.

* See Dr. Schœnbein's papers in Lond. and Edinb. Phil. Mag., vol. ix. p. 53, and several subsequent volumes.

"Experimental Researches on the mode of operation of Poisons." By James Blake, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

In this paper the author examines more particularly the action of those poisons which appear to produce death by affecting the nervous system.

After reviewing the evidence adduced in support of the opinion, that the effects of some poisons are owing to an impression made on the nerves of the part to which they are directly applied, he proceeds to relate a series of experiments undertaken in order to show with what rapidity the blood is circulated through the body, and tending to prove, that a substance may be generally diffused through the system in nine seconds after its introduction into the veins.

Experiments are then related in which the more rapidly fatal poisons had been used, and in which it was found, that an interval of more than nine seconds always elapsed, between the administration of a poison, and the appearance of the first symptoms of its action. The mere contact of a poison with a large surface of the body appears to be insufficient to give rise to general effects, as long as it is prevented from entering into the general circulation.

Various causes of fallacy in experiments of a similar kind, which have been adduced in support of an opposite opinion, are pointed out. The following is a summary of the conclusions arrived at by the author:—

1. The time required for a substance to penetrate the capillary vessels, may be considered as inappreciable.

2. The interval elapsing between the absorption of a substance by the capillaries, and its general diffusion through the body, may not exceed nine seconds.

3. An interval of more than nine seconds always elapses between the introduction of a poison, into the capillaries, or veins, and the appearance of its first effects.

4. If a poison be introduced into a part of the vascular system nearer the nervous centres, its effects are produced more rapidly.

5. The contact of a poison with a large surface of the body is not sufficient to give rise to general symptoms, as long as its diffusion through the body is prevented.

June 20.—The following papers were read: viz.

1. "Inquiries concerning the Elementary Laws of Electricity." Third Series. By W. Snow Harris, Esq., F.R.S.*

The author states, that it has been his object, in this series of investigations, to perfect the methods of electrical measurement, whether relating to the quantity of electricity, intensity, inductive power, or any other element requiring an exact numerical value, and by operating with large statical forces both attractive and repulsive, to avoid many sources of error inseparable from the employment of extremely small quantities of electricity, such as those affecting the delicate balance used by Coulomb. He then describes some im-

* An abstract of Mr. Harris's First Series will be found in Lond. and Edinb. Phil. Mag., vol. iv. p. 436.

provements in his hydrostatic electrometer, an instrument already mentioned in his first paper, which, although not available for the measurement of such minute forces as those to which the balance of torsion is applicable, is still peculiarly delicate and well adapted to researches in statical electricity. Its indications depending on the force between two opposed planes operating on each other under given conditions, are reducible to simple laws, and are hence invariable and certain; the attractive force between the discs is not subject to any oblique action, is referable to any given distance, and may be estimated in terms of a known standard of weight. The author next proceeds to the further consideration of the subject of his former papers, viz. the elementary laws of electrical action. He proves, by the following experiments, that induction invariably precedes, or at least accompanies attraction and repulsion.

A circular disc of gilded wood, about six inches in diameter, is suspended by an insulating thread of varnished silk from a delicate balance; a delicate electroscope is attached to this disc, and the whole is counterpoised by a weight. A similar disc insulated on a glass rod, and having also an electroscope attached to it, is placed at any convenient distance immediately under the former. One of the lower discs being charged with either electricity and the other remaining insulated and neutral, the electroscope of the neutral disc begins to rise, whilst that of the charged disc, already in a state of divergence, tends to collapse: when these respective effects ensue, the suspended disc descends the charged disc. Two inductive actions are indicated in this experiment, the one the author considers to be a direct induction, the other a reflected induction.

If the two discs are both charged with opposite electricities, on opposing them as before, the electroscopes begin to fall back, at which moment the discs appear to attract each other. But if the discs are both charged with the same kind of electricity, the divergence of the electroscopes increases, and at this instant the suspended disc recedes from that which is fixed, being apparently repelled by it.

The author proceeds to examine strictly the nature of this inductive influence, and adduces experiments to render probable that it is in some way dependent on the presence of an exquisitely subtile form of matter which may become disturbed in bodies, and assumes new states or conditions of distribution.

Very numerous experiments are detailed, showing the influence of changes of different intensity, of changes in the dimensions and distances of the opposed discs, of interposed bodies of different forms, &c. on the phenomena of induction. The author concludes by giving the following formulæ as the results of his investigations regarding the elementary laws of electrical induction and attraction. In these expressions $Q =$ quantity of charge, $T =$ the direct induction, $q =$ the quantity of electricity displaced, $t =$ its intensity, $T' =$ the reflected induction, $q' =$ the disturbed quantity, $t' =$ its intensity, $q'' =$ the total quantity in the opposed charged surface, $A =$ the

surface, D = the distance between the opposed points, F = the force of attraction.

For the direct induction:

$$T = \frac{Q}{\sqrt{D}} \qquad t = \frac{Q^2}{D}.$$

For the reflected induction:

$$T' = q' = \frac{Q}{D} \qquad t' = \frac{Q^2}{D^2} \qquad q'' = \frac{Q}{\sqrt{D}}.$$

For the attractive force between a charged and a neutral free conductor:

$$F = \frac{Q^2}{D^2} \qquad F = \frac{T}{A^2}.$$

For the force between two unchangeable surfaces, one positive the other negative:

$$F = \frac{Q^2}{D}.$$

2. "On the Conditions of Equilibrium of an Incompressible Fluid, the particles of which are acted upon by Accelerating Forces." By James Ivory, Esq., K.H., M.A., F.R.S., &c.

The intention of this paper is to examine the principles and methods that have been proposed for solving the problem of which it treats, with the view of obviating what is obscure and exceptionable in the investigation usually given of it.

The principle first advanced by Huyghens is clearly demonstrated, and is attended with no difficulty. This principle requires that the resultant of the forces in action at the surface of a fluid in equilibrium and at liberty, shall be perpendicular to that surface: and it is grounded on this, that the forces must have no tendency to move a particle in any direction upon the surface, that is, in a plane touching the surface.

In the Principia, Sir Isaac Newton assumes that the earth, supposed a homogeneous mass of fluid in equilibrium, has the figure of an oblate elliptical spheroid of revolution which turns upon the less axis: and, in order to deduce the oblateness of the spheroid from the relation between the attractive force of the particles, and their centrifugal force caused by the rotatory velocity, he lays down this principle of equilibrium, that the weights or efforts of all the small columns extending from the centre to the surface, balance one another round the centre. The exactness of this principle is evident in the case of the elliptical spheroid, from the symmetry of its figure: and it is not difficult to infer that the same principle is equally true in every mass of fluid at liberty and in equilibrium by the action of accelerating forces on its particles. In every such mass of fluid, the pressure, which is zero at the surface, increases in descending below the surface on all sides: from which it follows that there must be a point in the interior at which the pressure is a maximum. Now

this point of maximum pressure, or centre, is impelled equally in all directions by all the small columns standing upon it and reaching to the surface; and as the pressure in every one of these columns increases continually from the surface to the centre, it follows that the central point sustains the total effect of all the forces which urge the whole body of fluid. It follows also, from the property of a maximum, that the central point may be moved a little from its place without any variation of the pressure upon it: which proves that the forces at that point are zero. Thus the point of maximum pressure is in stable equilibrium relatively to the action of the whole mass of fluid: which establishes Newton's principle of the equiponderance of the central columns in every instance of a fluid in equilibrium and at liberty.

The two principles of Huyghens and Newton being established on sure grounds, the next inquiry is, whether they are alone sufficient for determining the figure of equilibrium. Of this point there is no direct and satisfactory investigation: and, in applying the two principles to particular cases, it has been found that an equilibrium determined by one, is not in all cases verified by the other; and even in some instances, that there is no equilibrium when both principles concur in assigning the same figure to the fluid. Further researches are therefore necessary to dispel the obscurity still inherent in this subject.

In a mass of fluid in equilibrium, if we suppose that small canals are extended from a particle to the surface of the mass, the particle will be impelled with equal intensity by all the canals: for, otherwise, it would not remain immoveable, as an equilibrium requires. It has been inferred that the equal pressures of the surrounding fluid upon a particle are sufficient to reduce it to a state of rest. Hence has arisen the principle of equality of pressure, which is generally admitted in this theory. Now, if the matter be considered accurately, it will be found that the only point within a mass of fluid in equilibrium which is at rest by the sole action of the surrounding fluid, is the central point of Newton, or the point of maximum-pressure. The reason is that, on account of the maximum, the pressure of all the canals terminating in the central point, increases continually as the depth increases; so that, besides the pressures of the canals, there is no other cause tending to move the particle. With respect to any other particle, the pressure caused by the action of the forces in some of the canals standing upon the particle, will necessarily increase at first in descending below the surface, and afterwards decrease; so that the effective pressure transmitted to the particle, is produced by the action of the forces upon a part only of the fluid contained in such canals. If a level surface be drawn through any particle, it is proved in the paper, that the equal pressures of the surrounding fluid on the particle, are caused solely by the forces which urge the portion of the fluid on the outside of the level surface, the fluid within the surface contributing nothing to the same effect. Thus a particle in a level surface is immoveable by the direct and transmitted action of the fluid on the outside of the level

surface; but it will still be liable to be moved from its place unless the body of fluid within the level surface have no tendency to change its form or position by all the forces that act on its own particles.

What has been said not only demonstrates the insufficiency of the principle of equality of pressure for determining the figure of equilibrium of a fluid at liberty, but it points out the conditions which are necessary and sufficient for solving the problem in all cases. The pressure must be a maximum at a central point within the mass: it must be zero at the surface of the fluid: and, these two conditions being fulfilled, there will necessarily exist a series of interior level surfaces, the pressure being the same at all the points of every surface, and varying gradually from the maximum quantity to zero. Now all the particles in the same level surface have no tendency to move upon that surface, because the pressure is the same in all directions: wherefore if we add the condition that every level surface shall have a determinate figure when one of its points is given, it is evident, both that the figure of the mass will be ascertained, and that the immobility of the particles will be established.

Maclaurin's demonstration of the equilibrium of the elliptical spheroid will always be admired, and must be instructive from the accuracy and elegance of the investigation. That geometer was the first who discovered the law of the forces in action at every point of the spheroid; and it only remained to deduce from the known forces the properties on which the equilibrium depends. These properties he states as three in number; and of these, the two which relate to the action of the forces at the surface and the centre of the spheroid, are the same with the principles of Huyghens and Newton, and coincide with two of the conditions laid down above. The third property of equilibrium, according to Maclaurin, consists in this, that every particle is impelled equally by all the rectilineal canals standing upon it and extending to the surface of the spheroid. Now it does not follow from this property that a particle is reduced to a state of rest within the spheroid, by the equal pressures upon it of the surrounding fluid: because these pressures may not be the effect of all the forces that urge the mass of the spheroid, but may be caused by the action of a part only of the mass. Maclaurin demonstrates that the pressure impelling a particle in any direction is equivalent to the effort of the fluid in a canal, the length of which is the difference of the polar semi-axes of the surface of the spheroid and a similar and concentric surface drawn through the particle, which evidently implies both that the pressures upon the particle are caused by the action of the fluid between the two surfaces, and likewise that the pressures are invariably the same upon all the particles in any interior surface, similar and concentric to the surface of the spheroid. Such surfaces are therefore the level surfaces of the spheroid; and every particle of the fluid is at rest, not because it is pressed equally in all directions, but because it is placed on a determinate curve surface, and has no tendency to move on that surface on account of the equal pressures of all the particles in contact with it on the same surface Maclaurin seems ultimately to have

taken the same view of the matter, when he says that* "the surfaces similar and concentric to the surface of the spheroid, are the level surfaces at all depths." It thus appears that the conditions laid down above as necessary and sufficient for an equilibrium, agree exactly with the demonstration of Maclaurin, when the true import of what is proved by that geometer is correctly understood.

The general conditions for the equilibrium of a fluid at liberty being explained, the attention is next directed to another property, which is important, as it furnishes an equation that must be verified by every level surface. If we take any two points in a fluid at rest, and open a communication between them by a narrow canal, it is obvious that, whatever be the figure of the canal, the effort of the fluid contained in it will be invariably the same, and equal to the difference of the pressures at the two orifices. As the pressure in a fluid in equilibrium by the action of accelerating forces, varies from one point to another, it can be represented mathematically only by a function of three co-ordinates that determine the position of a point: but this function must be such as is consistent with the property that obtains in every fluid at rest. If a, b, c, and a', b', c', denote the co-ordinates of the two orifices of a canal; and $\phi\ (a, b, c)$ and $\phi\ (a', b', c')$ represent the pressures at the same points; the function $\phi\ (a, b, c)$ must have such a form as will be changed into $\phi\ (a', b', c')$, through whatever variations the figure of a canal requires that a, b, c must pass to be finally equal to a', b', c'. From this it is easy to prove that the co-ordinates in the expression of the pressure must be unrelated and independent quantities. The forces in action are deducible from the pressure; for the forces produce the variations of the pressure. As the function that stands for the pressure is restricted, so the expressions of the forces must be functions that fulfil the conditions of integrability, without which limitation an equilibrium of the fluid is impossible. Thus, when the forces are given, the pressure may be found by an integration, which is always possible when an equilibrium is possible: and as the pressure is constant at all the points of the same level surface, an equation is hence obtained that must be verified by every level surface, the upper surface of the mass being included. But although one equation applicable to all the level surfaces may be found in every case in which an equilibrium is possible, yet that equation alone is not sufficient to give a determinate form to these surfaces, except in one very simple supposition respecting the forces in action. When the forces that urge the particles of the fluid, are derived from independent sources, the figure of the level surfaces requires for its determination as many independent equations as there are different forces.

In the latter part of the paper the principles that have been laid down are illustrated by some problems. In the first problem, which is the simplest case that can be proposed, the forces are supposed to be such functions as are independent of the figure of the fluid, and are completely ascertained when three co-ordinates of a point are

* Fluxions, § 640.

given. On these suppositions all the level surfaces are determined, and the problem is solved, by the equation which expresses the equality of pressure at all the points of the same level surface.

As a particular example of the first problem, the figure of equilibrium of a homogeneous fluid is determined on the supposition that it revolves about an axis and that its particles attract one another proportionally to their distance. This example is deserving of attention on its own account; but it is chiefly remarkable, because it would seem at first, from the mutual attraction of the particles, that peculiar artifices of investigation were required to solve it. But in the proposed law of attraction, the mutual action of the particles upon one another is reducible to an attractive force tending to the centre of gravity of the mass of fluid, and proportional to the distance from that centre: which brings the forces under the conditions of the first problem.

The second problem investigates the equilibrium of a homogeneous planet in a fluid state, the mass revolving about an axis, and the particles attracting in the inverse proportion of the square of the distance. The equations for the figure of equilibrium are two; one deduced from the equal pressure at all the points of the same level surface; and the other expressing that the stratum of matter between a level surface and the upper surface of the mass, attracts every particle in the level surface in a direction perpendicular to that surface. No point can be proved in a more satisfactory manner than that the second equation is contained in the hypothesis of the problem, and that it is an indispensable condition of the equilibrium. Yet, in all the analytical investigations of this problem, the second equation is neglected, or disappears in the processes used for simplifying the calculation and making it more manageable: which is a remarkable instance of attempting to solve a problem, one of the necessary conditions being omitted.

The equations found in the second problem, are solved in the third problem, proving that the figure of equilibrium is an ellipsoid.

3. "Report of a Geometrical Measurement of the Height of the Aurora Borealis above the Earth." By the Rev. James Farquharson, LL.D., F.R.S.*

The principal object to which the author directed the inquiries of which he here gives an account, is the determination by geometrical measurement of the height of the aurora borealis, and of the altitude and azimuth of the point to which the streamers seem to converge, and which has been termed the *centre of the corona*: these latter determinations constituting important data for enabling us to form a clear conception of the whole definite arrangement and progress of the meteor, and also a correct judgement of the degree of reliance to be placed on the methods employed for measuring its height above the earth. The paper is chiefly occupied with the details of the observations made or collected by the author, with their critical discussion, with the correction of some misapprehensions which have

* A notice of a former paper on this subject, by the same author, will be found in Phil. Mag. and Annals, N.S., vol. vii. p. 355.

existed respecting the views stated by the author in his former papers, and with a reply to the strictures of M. Arago on those views.

The result of the geometrical measurement of one particular aurora, gave as the height of its upper edge, 5693 feet above the level of the Manse at Alford; and the vertex of its arch was found to be 14,831 feet northward of the same place. The vertical extension of the fringe of streamers was 3212 feet; leaving 2481 feet for the height of the lower edge above the level of Alford. The tops of the Corean hills, immediately under the aurora, are about 1000 feet higher than that level; so that the lower edge of the arch was only 1500 feet above the summit of that range of hills.

4. "On the Phosphates." By John Dalton, D.C.L., F.R.S., &c.

The author takes a review of the labours of preceding chemists which bear upon the subject of the atomic constitution of phosphoric acid, and the salts in which it enters as a constituent; and shows their conformity with the views he has already advanced on the subject. A supplement is added, giving an account of the effects of various degrees of heat on the salt denominated the *pyrophosphate of soda.*

5. "On the Arseniates." By the Same.

The author here examines the conformity of the results of the analysis of the salts of arsenic with his theory, in the same manner as he has done with the phosphates in the preceding paper.

6. "On the Constitution of the Resins." Parts II. and III. By J. F. W. Johnston, Esq., F.R.S.*

In this paper the author, pursuing the train of investigation of which he has already given an account in a former communication, gives tabulated results of his chemical examination of several varieties of gamboge, and formulæ expressing their chemical constitution. A detailed account is given of the properties of the gambodic acid, and of the salts it forms with various bases, such as the gambodiates of potash and soda, of ammonia, and of different earths and metals, particularly lime, strontia, magnesia, lead, copper, zinc, and silver. He concludes from this investigation that the most probable formula for gamboge is $C_{40}\ H_{23}\ O_8$. In the analysis, however, of every specimen, there occurred a deficiency of carbon, amounting to nearly one per cent.; a deficiency supposed to be due to a change produced during the preparation of the natural resin for the market. By a heat of 400° Fahr. gamboge undergoes a partial decomposition; a resin, soluble in alcohol, and another resin, insoluble in that menstruum being formed: the formula representing the latter being $C_{40}\ H_{22}\ O_9$. Gamboge forms with the metallic oxides numerous salts, the existence and constitution of which, however, the experiments of the author only render probable.

The inquiries of the author were next directed to the chemical constitution of the resin of guaiacum, and to the properties of the salts it forms with various bases. He then examines the *acaroid resin,* which exudes from the *Xanthorrhœa hastilis,* and is often

* See our last volume, p. 340.

known by the name of *Botany-bay resin*, or *yellow gum*; and finds its formula to be $C_{40}\ H_{20}\ O_{12}$, showing that it contains more oxygen than any other resinous substance hitherto analyzed.

The general conclusions drawn by the author from these researches are the following.

1. Many of the resins may be represented by formulæ exhibiting their elementary constitution, and the weight of their equivalents, in which 40 C is a constant quantity.

2. There appear to be groups, in which the equivalents, both of carbon and the hydrogen, are constant, the oxygen only varying; and others, in which the hydrogen alone varies, the two other elements being constant.

In the third part of the same series of investigations, the author examines the constitution of the resin of Sandarach of commerce, which he finds to consist of three different kinds of resin, all of which possess acid properties. In like manner he finds that the resin of the *Pinus abies*, or spruce fir, commonly called *Thus*, or ordinary *Frankincense*, consists of two acid resins; the one easily soluble in alcohol, the other sparingly soluble in that menstruum. The gum resin *olibanum*, of commerce, was found to consist of a mixture of at least two gum resins, the resinous ingredient of each of which differs from that of the other in composition and properties.

7. "On the Markings of the Eel-back Dun variety of the Horse, common in Scotland;" in a letter to P. M. Roget, M.D., Sec. R.S. By W. Macdonald, M.D., Fellow of the Royal College of Physicians of Edinburgh, F.R.S. Ed., F.L.S., &c. Communicated by Dr. Roget.

The author states some obsèrvations which he has made on the coloured marks apparent in a variety of the horse, common in Scotland, and there called the *Eel-back Dun*, and which afford grounds for doubting the accuracy of the conclusions deduced in a paper, by the late Earl of Morton, published in the Philosophical Transactions for 1820. The title of the paper referred to is "A Communication of a singular fact in Natural History," namely, that a young chestnut mare of seven-eighths Arabian blood, after producing a female hybrid by a male quagga, had subsequently produced, by a fine black Arabian horse, a filly and a colt, both of which had the character of the Arabian breed as decidedly as could be expected where fifteen-sixteenths of the blood are Arabian, but in colour, in the hair of their manes, and the markings of the back and legs, bore a striking resemblance to the quagga.

The author, finding that similar markings are very commonly met with on the Eel-back dun ponies of Scotland, suggests, that as the breed of the mare in question was not pure she may have inherited the tendency to those peculiar markings. He moreover observes, that the cross-bar markings on the legs are not found in the *quagga*, but only in the *zebra*, which is a species quite distinct from the quagga; a fact which he considers as completely overturning the reasoning by which the conclusions stated in Lord Morton's paper

were deduced. The facts, he thinks, admit of a more natural explanation, and one more consistent with the known physiological laws of developement, by supposing the stain in the purity of the mare's Arab blood to have arisen from the circumstance of an early progenitor of the mare having belonged to the Eel-backed dun variety, the peculiarities of which re-appeared in a later generation.

8. "On the Structure and Functions of the Spleen." By Thomas Gordon Hake, M.D. Communicated by Francis Kiernan, Esq., F.R.S.

The author, passing in review the various opinions which have been advanced by anatomists respecting the intimate structure of the spleen, arrives at the conclusion that hitherto only vague and premature inductions have been made. It is generally admitted that the fibrous envelope of this organ is formed of the external fibres of the splenic vein; and that from the internal surface of this envelope fibrous prolongations are continued into the interior of its substance, giving support to a fine cellular membrane, which is continuous with their edges, and variously reflected so as to constitute cells. The parenchyma, or solid structure of the spleen, everywhere accompanies these membranous productions, and forms the exterior walls of the cells; being composed of branches of the splenic arteries, of the granular terminations of those arteries constituting the *splenic grains* of Malpighi, of *venules*, which ramify around the splenic grains, and of *cellules*, into which the venules open, and from which the splenic veins take their rise. The author concludes, as the result of his inquiries, that a dilatable cellular tissue exists, containing venous blood, between the granules within which the arteries terminate, and the venules on the outer side of the splenic grains: that the venous membrane, which is continued from the cells to the cellules, as well as to the venules, becoming more and more attenuated, but without changing its essential structure, gradually loses its tubular form, and resumes its primitive character of cellular tissue; and that the artery, in like manner, is limited in its distribution within the granules by a cellular structure, which becomes vicarious of it, and determines the function it has to perform.

The author, in conclusion, offers some observations on the probable functions of the spleen. He considers the opinion which supposes that organ to be distended, at particular times, with arterial blood, as being completely refuted by the evidence derived from the preceding account of its minute structure; and suggests the probability of the spleen being rather a diverticulum for venous blood.

The paper is accompanied by seven highly finished drawings illustrating the structures described.

9. "Additional Experiments on the formation of Alkaline and Earthy Bodies by chemical action when carbonic acid is present." By Robert Rigg, Esq., F.R.S.

The author gives a detailed account of several experiments in which sugar, water, and yeast only were employed, and from which he deduces the conclusion that alkaline and earthy matters are formed by chemical action. In one set of experiments, some of which

were made in silver, others in china, and others in glass apparatus, after the vinous fermentation had gone on during five days, the quantity of ashes obtained was, in the silver apparatus eighteen, in the china nineteen, and in the glass fifteen times greater than the previous quantity. A further examination of these ashes showed that they consisted of potass, soda, lime, and a residue not acted upon by muriatic acid. The author states that, however irreconcilable to our present chemical knowledge this important conclusion may at first sight appear, yet when it is taken in connexion with the decomposition of other vegetable matter, and with the phenomena which accompany the growth of plants, it may not excite surprise; and may be regarded as in harmony with the phenomena of natural science. He concludes by offering suggestions towards extending the inquiry into the subject of the formation of bones of animals by the action of the powers inherent in their organization.

10. "On the Difference of Colour in different parts of the Bodies of Animals." By James Alderson, M.A., M.D., late Fellow of Pembroke College, Cambridge. Communicated by P. M. Roget, M.D., Sec. R.S., &c.

The hypothesis advanced by the author in explanation of the well-known partial absence of the coloured pigment or *rete mucosum*, in different parts of the human body, and that of other animals, is that it is due to the union or adhesion of the epidermis and the true skin, so as to exclude the rete mucosum. He supports this hypothesis by the analogy of a cicatrix, which is the result of an organization of a certain portion of lymph, poured out from the cut surfaces of a wound, as part of the process of nutrition, or as the consequence of a small amount of inflammation, induced either from mechanical irritation, or other accidental circumstance. This hypothesis was suggested by the colourless appearance of the cicatrix from the section of the umbilical cord in the negro, and also of that seen by the author at the umbilicus of the bottle-nosed whale, the *Hyperoodon bidentatus*.

XLIX. *Intelligence and Miscellaneous Articles.*

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE: NINTH MEETING (AT BIRMINGHAM.)

THE Ninth Meeting of the British Association was held at Birmingham, from the 24th to the 31st of August, and was attended by more than fourteen hundred members; the Rev. W. V. Vernon Harcourt presiding. The Presidents of the Sections were as follows: Section A. *Mathematical and Physical Science*, Rev. Prof. Whewell; Sect. B. *Chemistry and Mineralogy*, Prof. Graham; Sect. C. *Geology and Physical Geography*, Rev. Dr. Buckland; Sect. D. *Zoology and Botany*, Prof. Owen; Sect. E. *Medical Science*, Dr. Yelloly; Sect. F. *Statistics*, Mr. H. Hallam; Sect. G. *Mechanical Science*, Prof. Willis. Mr. F. Baily was elected a Trustee of the Association, in the room of Mr. Babbage, resigned. Major Sabine

was elected one of the General Secretaries, in the room of the Dean of Ely (the Rev. G. Peacock), resigned. The following members were chosen to form the Committee of Recommendations:—Dr. Buckland, Dr. Daubeny, Prof. Forbes, Prof. Graham, Mr. Gray, Mr. Greenough, Mr. Hallam, Dr. Hodgkin, Mr. Hutton, Prof. Lloyd, Mr. Lyell, Marquis of Northampton, Mr. Porter, Dr. Roget, Mr. Fox Talbot, Prof. Wheatstone, Prof. Whewell, Mr. Willis, Dr. Yelloly. Of the numerous communications made to the Sections, three will be found in our present number, for which we are indebted to their authors, Mr. Lyell, the Rev. D. Williams, and Mr. Grove.

The next Meeting of the Association will be held at Glasgow, commencing on Thursday the 17th of September, 1840, the General Committee to assemble on the preceding day. The following noblemen and gentlemen have been chosen as officers for the next meeting:—The Marquis of Bredalbane, *President*; Dr. MacFarlane, Principal of Glasgow University, Lord Greenock, Sir T. M. Brisbane, Sir D. Brewster, *Vice-Presidents*; L. P. Nicholl, LL.D., A. Little, Esq., J. Strang, Esq., *Local Secretaries*; C. Forbes, Esq., *Local Treasurer*.

A full account of the proceedings of the late meeting, with abstracts of the principal reports and communications, has appeared in the *Athenæum*, August 31.—Sept. 21, 1839.

ON THE MOULTING PROCESS IN THE CRAY FISH.

We have extracted the following interesting notice from the elegant and valuable work of Prof. Rymer Jones*. "The phænomena which attend the renovation of the external skeleton are so unimaginable that it is really extraordinary how little is accurately known concerning the nature of the operation. The first question which presents itself is, how are the limbs liberated from their confinement? for, wonderful as it may appear, the joints even of the massive *chelæ* of the lobster do not separate from each other, but notwithstanding the great size of some of the segments of the claw, and the slender dimensions of the joints that connect the different pieces, the cast-off skeleton of the limb presents exactly the same appearance as if it still encased the living member. The only way of explaining the circumstance, is to suppose that the individual pieces of the skeleton, as well as the soft articulations connecting them, split in a longitudinal direction, and that, after the abstraction of the limb, the fissured parts close again with so much accuracy that even the traces of the division are imperceptible. But this is not the only part of the process which is calculated to excite our astonishment: the internal calcareous septa from which the muscles derive their origins, and the tendons whereby they are inserted into the moveable portions of the outer shell, are likewise stated to be found attached to the exuviæ; even the singular dental apparatus situated in the stomach, of which we shall speak hereafter, is cast off and re-formed!

* "General Outline of the Animal Kingdom," Part VII. September 1839.

And yet, how is all this accomplished? how do such parts become detached? how are they renewed? We apprehend that more puzzling questions than these can scarcely be propounded to the physiologist, nor could more interesting subjects of inquiry be pointed out to those whose opportunities enable them to prosecute researches connected with their elucidation."

In a note annexed to this paragraph he describes the appearances of an *Astacus fluviatilis*, which he had obtained soon after casting its shell, and of its newly cast-off covering. "All the pieces of the exuvium are connected together by the old articulations, and accurately represent the external form of the complete animal; the *carapace*, or dorsal shield of the cephalo-thorax alone being detached, having been thrown off in one piece. The pedicles of the eyes and external corneæ, as well as the antennæ, remain *in situ*, the corresponding parts having been drawn out from them as the finger from a glove, and no fissure of the shell or rupture of the ligaments connecting the joints is anywhere visible in these portions of the skeleton. The ordinary tubercles, and the membrane stretched over the orifice of the ear, occupy the same position as in the living crayfish. The jaws, foot-jaws, and ambulatory feet retain their original connections, with the exception of the right *chela*, which had been thrown off before the moult began; and the segments of the abdomen, false feet, and tail-fin exactly resembled those of the perfect creature;—even the internal processes derived from the thoracic segments (*apodemata*) rather seemed to have had the flesh most carefully picked out from among them than to have been cast away from a living animal: but perhaps the most curious circumstance observable was, that attached to the base of each leg was the skin which had formerly covered the branchial tufts, and which, when floated in water, spread out into accurate representations of those exquisitely delicate organs. No fissure was perceptible in any of the articulations of the small claws, but in the chela each segment was split in the neighbourhood of the joints and the articulated ligaments ruptured. The lining membrane of the stomach was found in the thorax, having the stomachal teeth connected with it; from its position it would seem that the animal had dropped it into the place where it lay before the extrication of its limbs was quite accomplished. The internal tendons were all attached to the moveable joint of each pair of forceps, both in the chela and in the two anterior pairs of smaller ambulatory legs.

"On examining the animal, which had extricated itself from the exuvium described above, the shell was found soft and flexible, but contained a sufficiency of calcareous matter to give it some firmness, especially in the claws. The tendons of the forceps were still perfectly membranous, presenting a very decided contrast when compared with the old ones affixed to the discarded shell. The stump of the lost chela had not as yet begun to sprout, and the extremity was covered by a soft black membrane. The jaws were quite hard and calcified, as likewise were the teeth contained in the stomach."

Route from Lima by the Quebrada of San Mateo. By John MacLean, Esq. of Lima. Communicated by the Hon. and Rev. Wm. Herbert.

Date.	Names of Places.	Thermometer at the period of observation.		Height in feet.	Hygrometer.	Boiling water.	Distance in Spanish leagues.		Productions.
1838.								Total.	
Dec. 8.	To Chaclacago	61·71	6 P.M.	2,265	24	207·5	...	6	Pasture, maize, and lucerne.
9.	San Pedro, Mama, and Santa Olaya	......	....	....	......	....	3	9	Much fruit and do.
	Cocachacra	·80	11 A.M.	5,331	27	203·6	3	12	Same but little.
10.	Surco	58·67	5 P.M.	6,900	......	200·4	3	15	Still less of either.
	Matucanas	·60	7 A.M.	8,026	......	....	1½	16½	Potatoes.
	Tambo de Viso	·62	9 A.M.	9,072	......	....	1¾	18¼	Ditto.
	San Mateo	·62	1 P.M.	10,984	......	....	2	20¼	Do. peas, ollneos, and marhoao.
11.	Checla	39·55	4 P.M.	12,712	......	191·8	2	22¼	Pasture for Llamas.
	Pass of the Cordilleras at Antaranan	·51	10 A.M.	15,543	......	....	4¼	26½	Strong tufts of grass on bare rocks.
12.	Pachachaca	35·60	4 P.M.	12,288	......	....	5	31½	Sheep pasture.
	Croya	·65	11 A.M.	12,010	......	....	3	34½	Ditto.
13.	Tarma	49·68	3 P.M.	9,183	15	195·6	6	40½	Lucerne, maize, and potatoes.
14.	Palcamago	·64	Noon.	10,284	......	....	5	45½	Wheat, potatoes, and many bulbs.
15.	Cacas	36·48	4 P.M.	12,280	......	....	2½	48	Sheep pasture.
17.	Ondores	39·56	2 P.M.	13,000	12	190·6	4½	52½	Do. and a few macas.
19.	Cerro de Pasco	37·55	11 A.M.	13,673	18	189·2	9½	62	Silver mines.
23.	Caxamarquilla	52·68	4 P.M.	10,866	......	....	5	67	Leagues from Lima viâ Tarma.
28.	Angascancha	·50	4 P.M.	13,323	......	....	...	1½	Llama pasture.
29.	Ninacaca	......	....	12,853	......	....	5½	7	Sheep do.
	Runamarca	·55	1 P.M.	14,589	......	....	...	......	High bare ridge: Llama pasture.
30.	Pancartambo	52·60	2 P.M.	8,586	......	196·3	7	14	Fine potatoes and maize.
31.	Huanacanchan	·63	10 A.M.	6,996	......	....	6	20	Near this the wood cutting.
	Ceja de la Montana	......	....	7,900	......	....	1	21	Leagues from Cerro de Pasco.

ACTION OF ÆTHER ON INDIGO.—BY M. VOGEL.

I had more than once remarked by chance that when the vapour of æther is passed into a solution of indigo in sulphuric acid largely diluted with water, it was decolorated. This effect is produced more readily when the æther is heated to ebullition in a matrass furnished with a bent tube which is immersed in the solution of indigo; and if the matrass be suddenly cooled, so that the solution of indigo rises in the tube and passes into the matrass by the pressure of the air. When I attempted on another occasion to decolorate indigo by means of æther which had been rectified over potash, I could not so readily effect it, which induced me to believe that the impure æther, which contained sweet oil of wine, or probably aldehyd, was more fit for the decoloration of indigo than pure æther.

To satisfy myself, I added to a solution of indigo in a bottle a few drops of aldehyd, and I remarked that the liquor, at first of an emerald green colour, became of a pale green, and after some days became of a yellowish brown. As the aldehyd which I employed contained alcohol, it not having been rectified, I afterwards made use of pure aldehyd, which was separated from its crystalline combination by ammonia: a few drops of this pure aldehyd were sufficient to destroy the blue colour of indigo in a very short time, the solution becoming of a straw yellow. When the aldehyd was evaporated by heat, the blue colour could not be made to reappear. The addition of potash, and of red oxide of mercury, were not capable of restoring the blue colour. On evaporating the decolorated liquor there remained a brown substance analogous to ulmin. This decoloration of indigo by aldehyd occurs only when the indigo is dissolved in sulphuric acid. Indigo in fine powder, diffused through water, undergoes no change by aldehyd: neither the tincture of litmus, nor the spirituous tinctures of cochineal or turmeric, are decolorated by aldehyd.—*Journal de Pharm. Mars* 1839.

EFFECTS OF MUSHROOMS ON THE AIR.

According to Dr. Mariet mushrooms produce very different effects upon atmospheric air, from those occasioned by green plants under the same circumstances; the air is promptly vitiated, both by absorbing oxygen to form carbonic acid at the expense of the vegetable carbon, or by the evolution of carbonic acid immediately formed; the effects appear to be the same both day and night.

If fresh mushrooms be kept in an atmosphere of pure oxygen gas, a large proportion of it disappears in a few hours. One portion combines with the carbon of the vegetable to form carbonic acid, and another is fixed in the plant, and is replaced by azotic gas disengaged from the mushrooms.

When fresh mushrooms are placed for some hours in an atmosphere of azotic gas, they produce but little effect upon it. A small quantity of carbonic acid is disengaged, and in some cases a little azote is absorbed.—*Ibid.*

STARS AT PRESENT UNDISCOVERABLE IN THE HEAVENS.

The following is an addendum to the Introduction to the Greenwich Observations for 1837, p. lxviii.

Addendum to the Introduction.—The following stars have been repeatedly sought in the heavens, but no traces of them are discoverable :—

A. S. C. 337
805
2460

The stars L and C′, observed at Cambridge with Halley's comet (Camb. Obs. 1835.). The following stars observed by Sir John Herschel with Halley's comet (Ast. Soc. Mem. vol. x.):—

	h. m. s.		o. s.
A. R.	10 12 10	N. P. D.	99 17
	15 38 31		119 30
	15 41 4		119 16?
	15 42 39		119 6

METEOROLOGICAL OBSERVATIONS FOR AUGUST, 1839.

Chiswick.—Aug. 1. Fine. 2, 3. Hot. 4—6. Very fine. 7. Rain, with thunder at night. 8. Overcast and fine. 9—14. Very fine. 15. Hazy: drizzly. 16. Very fine: cloudy: rain at night. 17. Rain. 18. Very fine: heavy rain at night. 19. Rain. 20. Hazy: fine. 21. Clear and fine. 22—26. Very fine. 27. Overcast: slight rain. 28. Hazy. 29. Cloudy: rain at night. 30. Rain: fine. 31. Cloudy: rain.

Boston.—Aug. 1. Fine: rain early A.M. 2, 3. Fine. 4. Cloudy: rain P.M. 5, 6. Fine. 7. Rain. 8, 9. Fine. 10. Fine: rain P.M. 11. Rain. 12. Cloudy. 13. Fine. 14. Rain. 15. Fine. 16. Cloudy: rain early A.M.: rain, thunder and lightning P.M. 17. Fine: rain A.M. and P.M. 18. Fine. 19. Rain: extraordinary rain early A.M. 20. Cloudy: rain A.M. and P.M. 21, 22. Fine. 23. Cloudy. 24. Cloudy: rain early A.M. 25. Fine. 26. Cloudy. 27. Fine: rain early A.M. 28. Cloudy: rain P.M. 29. Cloudy. 30. Cloudy: rain early A.M. and P.M. 31. Rain: rain early A.M. and P.M.

Applegarth Manse, Dumfries-shire.—Aug. 1. Pleasant day: getting cloudy P.M. 2. Rain nearly all day. 3. Calm and temperate: cloudy P.M. 4. Fine clear day. 5. Fine: at noon sultry: air electrical. 6. Wet all day. 7. Occasional showers. 8. Fine: pleasant breeze: sky clear. 9. Slight rain A.M.: cleared up. 10. High wind: dry A.M.: showery P.M. 11. Fair and fine A.M.: showery P.M. 12. Dull, but fair. 13. Clear and calm all day. 14. Very wet from 11 A.M. 15. Damp and drooping all day. 16. Occasional drizzling all day. 17. Dry and partially clear. 18. Warm and close: showery P.M. 19. Dropping day. 20. Chill morning: fair: showery P.M. 21. Fair throughout: hoar frost A.M. 22. Fine day: heavy dew A.M. 23. Rain at noon and continued all day. 24. Dropping day. 25. Fair till afternoon: cloudy and close. 26. Fair throughout. 27. Beautiful harvest day. 28. Fair A.M.: came on heavy rain P.M. 29. Heavy rain: flood in the river. 30. Fine day: occasionally slight drizzle. 31. Very wet till 5 P.M., when it cleared.

Sun 27 days. Rain 18 days.

Wind southerly 18 days. Northerly 8 days. Westerly 4 days. Easterly 1 day.

Calm 15 days. Moderate 8 days. Brisk 4 days. Strong breeze 2 days. Boisterous 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. August.	Barometer.						Thermometer.									Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Min.	Max.	Min.		9 a.m.	9 p.m.									
☾ 1.	29·974	30·085	29·939	29·38	29·86	29·86	60·6	66·4	54·4	76	50	59	60	60	W.	SW.	NW.	S.	·213	...	·05	...	57
2.	30·120	30·095	30·049	29·52	29·89	29·92	65·3	76·2	56·2	82	53	66	60	56	S.	S.	SW.	S.	...	...	...	...	59
3.	30·096	30·138	30·063	29·43	29·94	30·10	68·8	74·4	59·9	85	57	70	61	58½	WNW.	SW.	calm	S.	...	...	...	...	61
4.	30·222	30·325	30·204	29·60	30·20	30·30	62·0	78·6	61·3	74	46	62	60	55	NNW.	W.	calm	W.	...	...	...	...	61
5.	30·348	30·296	30·200	29·71	30·28	30·20	61·8	71·0	52·6	74	49	60	61½	58	NW.	NE.	N.	W.	...	...	·02	...	59
6.	30·228	30·201	30·049	29·56	30·01	29·73	65·5	71·3	56·4	74	58	63	58½	56	S.	W.	calm	SW.	...	·01	...	1·12	60
7.	29·844	29·855	29·774	29·17	29·52	29·69	63·4	71·7	60·0	70	51	57·5	59	58½	SSW.	SW.	W.	SW.	...	·15	·15	...	59
8.	29·998	30·026	29·965	29·34	29·72	30·02	61·2	71·2	53·7	74	47	63	60	52½	NW.	W.	W.	W.	·288	...	...	...	60
● 9.	30·188	30·147	30·100	29·54	29·94	29·85	64·0	76·3	53·9	75	53	62	56	58	SSW.	W.	calm	W.	...	...	...	...	59
10.	30·152	30·121	30·087	29·45	29·77	29·67	64·6	71·3	56·2	74	54	63	58½	55½	SW.	W.	W.	SW.	...	...	...	...	60
11.	30·060	30·252	30·043	29·35	29·82	30·15	63·7	70·2	55·9	70	48	59·5	57½	54½	SW. var.	NW.	W.	NW.	...	·01	·03	...	59
12.	30·314	30·287	30·263	29·72	30·24	30·20	59·3	67·0	53·2	71	49	55	56	55	NW.	NW.	calm	NE.	...	·04	·39	...	57
13.	30·208	30·198	30·081	29·65	30·23	30·02	58·3	62·6	53·3	70	52	58	56	58	NW.	NW.	calm	SE.	...	...	...	...	50
14.	30·014	30·001	29·951	29·44	29·93	29·80	63·8	70·0	57·3	70	54	57	52	52	SE.	S.	calm	E.	...	...	·05	0·96	58
15.	29·696	29·689	29·621	29·18	29·80	29·71	61·0	68·7	57·8	74	52	65	55	54	E.	E.	S.	SE.	...	·04	...	...	55
16.	29·654	29·632	29·619	29·07	29·67	29·69	64·3	65·0	56·9	77	54	63	56	53	S.	W.	calm	NE.	·019	·58	·07	...	59
☽ 17.	29·670	29·862	29·657	29·10	29·80	29·90	61·8	70·0	56·4	71	50	63	55	50½	S.	SW.	E.	NE.	·686	·08	·15	...	59
18.	30·030	30·030	29·988	29·45	29·99	30·00	69·3	67·8	52·9	71	53	59·5	59	51	N.	NE.	N.	NE.	...	·24	·08	...	57
19.	29·946	30·040	29·932	29·45	29·98	30·01	60·2	66·0	55·8	61	45	56	57	49	WNW.	NW.	E.	NE.	·347	·12	1·89	...	58
20.	30·080	30·205	29·968	29·59	30·11	30·17	52·5	59·4	49·4	63	37	52	53½	47	W.	N.	calm	N.	·133	·01	...	...	51
21.	30·244	30·221	30·207	29·74	30·18	30·16	54·2	56·3	45·4	66	39	51	54½	50	NW.	N.	N.	SW.	·025	...	·13	0·60	50
22.	30·230	30·247	30·213	29·72	30·13	30·14	57·7	60·2	49·7	73	55	56	56½	61	S.	SW.	calm	SW.	...	...	...	...	49
23.	30·266	30·246	30·184	29·67	30·15	30·06	62·4	65·3	57·2	77	49	63	60	57	S.	NW.	calm	S.	...	...	...	...	56
○ 24.	30·140	30·136	30·013	29·47	29·91	29·82	62·5	70·8	57·6	75	50	63	58	56	S.	SW.	calm	SSW.	...	...	·13	...	59
25.	30·026	30·001	29·971	29·38	29·83	29·79	62·3	70·5	60·0	77	46	64	57½	57	W.	W.	W.	SW.	...	...	...	...	59
26.	29·966	29·950	29·838	29·33	29·73	29·70	65·3	71·5	54·8	73	54	60·5	58	54	W.	SW.	calm	SW.	...	...	...	...	59
27.	29·818	29·958	29·792	29·20	29·69	29·88	68·8	70·3	55·8	70	45	58	56	57½	NW.	NW.	W.	NW.	...	·15	·07	...	58
28.	30·072	30·053	30·029	29·48	29·91	29·74	55·4	65·0	50·9	72	56	57	56	56½	SW.	NW.	calm	SW.	·066	...	...	1·40	54
29.	29·942	29·923	29·878	29·30	29·59	29·60	62·3	65·3	54·9	71	56	62	57	56	S.	SW.	SW.	SW.	...	·02	·12	...	60
30.	29·846	29·828	29·762	29·20	29·64	29·65	61·7	68·3	59·9	67	51	62	60	55½	SE.	S.	W.	SE.	·133	·10	·12	...	59
☾ 31.	29·530	29·544	29·172	28·98	29·29	29·03	61·8	66·8	57·0	63	55	57	53½	53½	SSW.	SW.	calm	SE.	·058	·30	·02	...	61
Mean.	30·030	30·051	29·955	29·42	29·892	29·888	62·1	68·6	55·4	72·25	50·58	60·2	57·6	55·					Sum. 1·968	1·85	3·47	4·08	Mean. 57·5

THE

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NOVEMBER 1839.

L. *On the Rule for finding the Value of an Annuity on three Lives.* By AUGUSTUS DE MORGAN, *Professor of Mathematics in University College.**

THE rule which was given by Thomas Simpson for finding the value of an annuity on three lives is as follows: Let A, B, and C be the three lives and let K be a single life, the annuity on which is equal to an annuity on the joint lives of B and C; then the value of an annuity on the joint lives of A and K is the required annuity on the joint lives of A, B, and C. The life A should be the youngest of the three.

No reason has ever been given for this rule, simple as it is; and the only considerations of which I know, partaking of the nature of demonstration, and connected with it, are those by which Mr. Milne has shown that A should be the youngest of the three, and also that, instead of interpolating the age of K to a fraction of a year, the complete year of age next above the real age of K should be chosen.

If the rule be examined by the hypothesis of equal decrements, and if $1-\alpha$, $1-2\alpha$, &c. be the chances of A's living one, two, &c. years, and $1-\beta$, &c. $1-\gamma$, &c. the same for B and C, the error of the rule, or at least its order, will be represented by the fraction

$$\frac{2\alpha\beta\gamma}{r^4}, \text{ (r = interest of £1 for one year)}$$

whenever α, β, γ are so small that $\alpha\beta\gamma$ is small compared with r^4. As far as my investigations go, I doubt if, on the hypothesis of equal decrements, the rule would be a good ap-

* Communicated by the Author.

proximation when $\alpha\beta\gamma$ is not a small fraction of r^4; and this may account for De Moivre not having noticed a rule which would, it might be imagined, present itself for trial at least, to any one who was considering the subject for the first time.

A much more satisfactory account of this rule can be given from the expression for the law of human mortality which was given to the Royal Society, by Mr. Benjamin Gompertz, in 1825. That law is as follows: The number living at x years from a given age (upwards of 10) may be represented by

$A g^{q^x}$ where $A g$ is the number living at the given age. The constants do not retain their value during the whole of life; Mr. Gompertz, for instance, finds one set of values representing the Carlisle tables very nearly from 10 to 60, and another from 60 to 100. The closeness with which this theory represents the Northampton, Sweden, Carlisle, and Deparcieux's tables may be seen in the paper cited; and it adds not a little to the speculative value of the formula, that its author has deduced it from so simple a principle as that *the power of the human constitution to oppose decay loses equal proportions in equal times.*

If Mr. Gompertz's theory were accurately true, with a uniform value of the constants, throughout the whole remainder of life, Thomas Simpson's rule would no longer be an approximation, but an exact method. Let $A g^{q^x}$ be the number living at the age of A, and let B be y years older than A, and C z years older than B. Consequently the numbers now alive in the table at the ages of B and C will be

$$A g^{q^{x+y}} \quad \text{and} \quad A g^{q^{x+y+z}} \qquad \text{or} \qquad A h^{q^x} \quad \text{and} \quad A k^{q^x}$$

$$\text{where} \qquad h = g^{q^y} \qquad k = g^{q^{y+z}}$$

Hence the chances of the parties living t years are

$$g^{q^x(q^t-1)}, \quad h^{q^x(q^t-1)}, \quad k^{q^x(q^t-1)}$$

and, v being the present value of £1 to be received in one year, the value of the annuity is

$$(g\,h\,k)^{q^x(q-1)} \cdot v + (g\,h\,k)^{q^x(q^2-1)} \cdot v^2 + \ldots\ldots \quad (1.)$$

Determine w and l from $h\,k = g^{q^w} = l$, then it will be evident that if (1.) be called $\phi\,(g\,h\,k)$, we have $\phi\,(g\,hk) = \phi\,(g\,l)$, the annuity on the joint life of A and another, whose

single life is worth $\phi\,(l)$ or $\phi\,(h\,k)$, the value of an annuity on the joint lives of B and C.

The various causes of error in the preceding, arising from the change in the values of the constants, will readily present themselves to those who have studied the subject.

It is not, of course, necessary that the progression of powers should be precisely that of Mr. Gompertz, and the following theorem may easily be demonstrated. If a_n be the chance of a life living n years, and if

$$a_n = (a_1)^{p_n},$$

where p_n is not a function of a, then the annuity on any number of lives is not altered in value if, instead of any part of those lives, a single life of equivalent value be substituted.

LI. *On the Use of a Secondary Wire as a Measure of the Relative Tension of Electric Currents.* By John W. Draper, *M.D., Professor of Chemistry in the University of New York; late Prof. of Physical Science in Hampden Sydney College, Virginia.*

[Continued from p. 279, and concluded.]

BEFORE however we can go further in the study of these conditions of tension, or attempt to show that the arrangement of Volta, and a single pair under the influence of a long or thin wire, are, in point of fact, alike in principle, it is necessary that we should understand the nature of the different disturbing actions that may arise in the generating cells of the electromotor.

I took a zinc plate 7 inches long and 3 wide, and a corresponding copper: the surface of the former was amalgamated and the latter brightened. The plates were fixed at an immovable distance from each other, and immersed in a jar containing 34 ounces of water. Sulphuric acid was then added by half drachms successively.

Table E.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	20	·555
2.	1	39	·436
3.	1½	55	·382
4.	2	72	·305
5.	2½	83	·277
6.	3	98	·245
7.	3½	112	·223
8.	4	121	·214
9.	8	216	·130

Here we have an exemplification of the converse of the fact already so much insisted on. As the quantity developed from the *same surface* gradually became greater and greater, the tension became less and less, due to the increased conducting powers of the fluid medium. It is the same effect that would have been produced by constantly shortening the conecting wire.

Such is the action of increasing doses of sulphuric acid; let us now see how NITRIC ACID will act. The copper plate being repolished, and the zinc reamalgamated, and everything else being as at the commencement of the former trial, the latter acid was now added to the cell, in the same way that the former had been used.

TABLE F.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	14	·7143
2.	1	22	·6363
3.	1½	36	·5555
4.	4	175	·2400

These measures are effected with some difficulty as the acid acts somewhat irregularly, and keeps the needle vibrating.

MURIATIC ACID, under the same conditions and circumstances, being substituted, gave as follows:

TABLE G.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	10	·8000
2.	1	17	·6530
3.	1½	23	·6087
4.	2	29	·5517
5.	2½	34	·5000
6.	3	39	·4872
7.	3½	44	·4654
8.	4	49	·4387
9.	8	82	·3169
10.	16	145	·1938
11.	24	203	·1707

NITROSULPHURIC ACID, the constituents of which were added alternately in equal measures, was next tried.

TABLE H.

Exp.	Acid.	Quantity in drachms.	Quantity.	Tension.
1.	Sulphuric.	½	23	.5652
2.	Nitric.	½	53	·5094
3.	Sulp.	1	72	·3888
4.	Nitr.	1	94	·3510
5.	Sulp.	2	178	·2360
6.	Nitr.	2	190	·2026

Solution of SULPHATE of COPPER was next experimented with.

TABLE I.

Exp.	Quantity.	Tension.
1.	33	8181
2.	74	6081
3.	116	4741

These measures were procured with difficulty, owing to the flocculent deposit which settled on the zinc, more rapidly as the solution was made stronger.

In the general discussion of the measures, given by tables E, F, G, H, I, we still see the operation of the same general law, that the tension rapidly diminishes as the acid is added, and that when the same quantity of electricity developed from the same amount of surface by these different acids is presented to the secondary wire, the quantities that can pass that wire are very different; and on making use of these different agents it would appear that they can give rise to currents from the same metalline surface, equal in point of quantity, but differing greatly in point of tension, in the following order, beginning with the most powerful:

Sulphate of copper,
Nitric acid,
Nitrosulphuric acid,
Muriatic acid,
Sulphuric acid.

Of these bodies the muriatic acid acts probably in the way that Dr. Faraday has pointed out, but the immediate cause of the rise of tension in the others, is to be traced to the circumstance, that they furnish oxygen to the nascent hydrogen; and if the tension of the ordinary current is made to depend on the tendency of the zinc and oxygen to unite, it is reasonable to suppose that that tension will increase if a new affinity be in-

troduced, the action of which should correspond with, and abet that of the zinc for oxygen. This takes place when nitric acid or an oxysalt of easy decomposibility is added to the solution. The tension of the current is not then determined by the affinity of the zinc for oxygen only, but by *all* the affinities that can take place, among *all* the bodies in the exciting cell. We are therefore here led to expand Dr. Faraday's theory, and to regard what follows as directly opposed to the theory of contact.

Upon these principles, in an ordinary arrangement of copper, zinc, and sulphuric acid, the tension of the current is determined; by the sum of the affinities of zinc for oxygen, and hydrogen for copper, diminished by the sum of the affinities of copper for oxygen, oxygen for hydrogen, and hydrogen for zinc.

But as under all ordinary circumstances the affinities of hydrogen for zinc and copper may be neglected, they being exceedingly small in comparison with the others, we may assume,—

That the tension of the current is equal to the affinity of oxygen for zinc, diminished by the sum of the affinities of hydrogen and copper respectively for oxygen.

If now we introduce into the exciting cells nitric acid or sulphate of copper, the affinity of the nascent hydrogen for oxygen is satisfied, and the resistance from this source is nearly exterminated, and the tension of the current is then equal to the difference of the affinities of zinc and copper for oxygen.

By thus exterminating the resistance arising from the affinity of hydrogen for oxygen, we succeed in raising the tension greatly; if next we get rid of the affinity of copper for oxygen, the tension ought to become still higher. This may in a measure be effected by making use of a plate of platina, as I found experimentally.

In all these cases, in which the tension increases without loss of quantity, we directly trace the action to a disturbance in the exciting cells. In ordinary voltaic arrangements, the maximum tension is never reached, because the affinity of zinc for oxygen, which determines the current, is counteracted to a certain extent by the affinity of oxygen for hydrogen. If we satisfy that affinity, an increase of tension is the result, and accordingly as this is more and more nearly effected, more and more of the hydrogen that ought to be evolved disappears. This remarkable disappearance of hydrogen has been heretofore noticed, but the true office it served has not been detected. If a battery is charged with nitrosulphuric acid,

the hydrogen evolved is no longer the equivalent of the zinc expended; in point of fact, the gas evolved is no longer hydrogen, but a mixture of hydrogen and the binoxide of nitrogen, as is proved by its burning with a green flame. I took a small pair of plates, the zinc being amalgamated and the platina freshly cleaned, and placed them in a mixture of six ounces of water and one drachm of sulphuric acid, arranging an inverted tube over them, so as to collect the gas from the platina plate. I determined by weighing the zinc plate how much was expended in evolving a given quantity of gas, and then successively adding sulphuric acid until the total amount had reached eight drachms, it appeared that in each instance it required very nearly 1·79 grains of metal. But on adding one drachm of nitric acid to the mixture, the quantity expended rose at once to 2·25 grains, and on adding a second to 3·00 grains.

Therefore, unless care is taken that no oxidizing body is present, the voltameter will give deceptive results. This important precept should be perpetually borne in mind by those who employ it in investigations. A few drops of nitric acid will at once vitiate its indications; and there is reason to suspect that under certain circumstances even the dilute sulphuric acid with which it is charged may undergo partial deoxidation, and the evolved hydrogen indicate an amount of electricity less than is actually passing.

We are therefore in possession of two distinct methods of indirectly increasing the tension of an electric current. The first depends on the reduction of quantity; the second on satisfying in the exciting cells, affinities which tend to antagonize that which determines the current.

Volta's plan of a reduplicated series unquestionably acts upon the first of these principles. It is a fact admitted on all hands, and therefore into the proof it is unnecessary now to go, that the apparent quantity circulating in the whole battery is not greater than that which any one of the pairs could generate. Dr. Faraday has already shown how an enormous quantity of zinc is thus expended, the equivalent of electricity being entirely sacrificed, for the sake of increasing the tension. Let us see what are the facts in the case. The first pair of plates developes by the oxidation of a portion of its zinc a certain quantity of electricity, which passing through the electrolytic conductor arrives at the second cell; here however it is stopped, as a transit without decomposition is impossible, a decomposition which it is unable to effect. Continually tending to pass, without the passage actually taking place, it remains as it were on the surface of the second zinc plate, in a condensed

state, reacting on the electricity which that plate is generating, compressing and being compressed by it, and therefore increasing its elastic force. And the same action continually occurs, and increases the tension throughout the series.

A flat spiral coil, or a long connecting wire, obviously acts in the very same way. It opposes a resistance to the passage of the current, and the plate instantly becomes in a forced state. We might almost regard the electric fluid as existing upon the surface of the zinc, exerting to the utmost its elastic force to pass the barrier, and failing that, compressing the evolved fluid as fast as it is generated, and being compressed by it. This also is the case in the pile of Volta.

Thus far therefore the ribbon coil acts simply as a long wire, and this may be regarded as its primary or statical effect. But besides this, it gives rise to an action of an entirely different character, which Prof. Henry pointed out and explained. In the act of making and breaking contact in a system of which it forms a part, Faradian currents are generated by its successive spirals; these currents under the latter condition, breaking contact, coincide in direction with the primary current then just ceasing to pass. We must however carefully distinguish between these currents and that which induced them. In this respect some philosophers have unguardedly fallen into a very remarkable mistake; it has been supposed, that when a thermo-electric current was passed through this coil, and a spark obtained, the thermal light was seen! The case is exactly analogous to that in which similar coils pass the jaws of a horseshoe magnet; no one supposes that the spark then elicited is due to the electricity of the magnet itself, but is simply a manifestation of the induced current: the very same thing takes place when the thermal current runs through the spires of a flat coil. So far as I am informed, the magnetic spark and the true thermo-electric spark have never yet been seen.

These observations are made, in order that I may not be misunderstood. It is not my object to consider the different arrangements that can generate a Faradian current, and therefore in this point of view I dismiss the flat spiral.

We now come to the fourth and last proposition, which is, "That the law which regulates the connexion of the diminution of quantity or condensation, with the increase of tension, is the same as that which regulates the analogous phænomena of ponderable elastic fluids."

I have not hesitated to use the terms 'compression,' 'condensation,' 'elastic force,' in reference to electricity, though well know such an application is unusual. But it has

seemed to me, that a single pair might *almost* be likened to a steam-engine boiler, from which if you let the steam escape by a wide tube, its elastic force is less and less, accordingly as the escape is more free; but if you put upon it a narrow tube, the vapour rushes with vehemence through it, reaction in a moment occurs in the boiler, the elastic force increases, and the accumulated steam pressing heavily on its surface, the water boils in a more laboured way: this narrow tube resembles Henry's coil, or a long or slender wire.

The following table exhibits numerical results, obtained by the aid of one of Daniell's constant batteries, the tension being continually increased by the addition of successively increasing lengths of wire.

Table K.

		Quantity.	Calculated.	Tension.
Without any wire interposed	Beginning of the experiment.	76	......	·8289
	End of ditto	75		
Wire interposed 6 feet long		72	72·00	·8333
.. .. 12 ..		68·50	68·30	·8394
.. .. 18 ..		65	65·24	·8461
.. .. 24 ..		62	62·30	·8548
.. .. 48 ..		53	53·12	·8679
.. .. 72 ..		46	46·18	·8913
.. .. 96 ..		41	41·00	·9219

From this table it would appear, that the addition of successively increasing lengths of wire of invariable diameter, diminishes the absolute quantity of electricity flowing, but at the same time the tension is exalted. By taking the angle of torsion as the measure of the forces, in the second column, it is also evident that the law of the conducting power of wires given by M. Lenz, holds in the case of a hydro-electric pair. This may be regarded as of some interest, in as much as the late Dr. Ritchie, in certain papers read before the Royal Society*, opposed to the very last this view, by the aid of numerical determinations made with the torsion balance, the instrument here employed. In reference to the third column of the table, I have calculated it in the manner given by Lenz, the value of the constant to be deduced from the reciprocals of the angles of torsion being in this case 1318 nearly.

Whilst therefore these results confirm in the most pointed manner the reasoning of that able philosopher, they at the same time compel us to advance a step further, and to expand

[* L. & E. Phil. Mag., vol. xi. p. 192. Edit.]

to a certain extent Ohm's theory of the voltaic pile. It is a condition, in tracing the action of wires of different lengths, to assume, that the electromotive power of the generating pair is under all circumstances constant, and hence it may be conveniently represented by unity. But the electromotive power of any pair plainly depends on *two* things, the quantity of electricity that the pair can evolve and its absolute tension. The theory of Ohm, as may be gathered from the memoir of Prof. Jacobi on electromotive machines, and also from M. Lenz's papers*, confounds those two important conditions.

Now the results given in the foregoing table, proving that wires conduct in the inverse ratio of their lengths, prove also that the addition of increasing lengths of wire does not in anywise alter the electromotive power; yet we have clearly shown that this addition is inevitably attended with an increase of tension. Here therefore is an apparent contradiction.

But this contradiction is only apparent, and when properly understood leads to a most remarkable result.

It is true, that we are compelled to assume that the electromotive power of a pair is independent of the length of the connecting wire; but this constancy of electromotive power does not necessarily imply that the relations of quantity and tension, which conjointly produce it, are not themselves variable. In the case before us, we have direct proof that the tension increases, and also that the quantity decreases, as the connecting wire becomes longer, and the converse; yet the electromotive power varying directly with them both, they must of necessity bear such a relation to each other, that their product shall always be equal to unity. Hence we infer,

That the law of Marriotte in relation to the ponderable elastic fluids, holds also in the case of electricity developed by voltaic action, the elastic force or tension of a given quantity being inversely as the space it occupies†.

[* See Taylor's Scientific Memoirs, vol. i. p. 311, 503, and vol. ii. p. 1. Edit.]

† The formula given by the German natural philosopher is,

$$F = \frac{E}{l + \lambda}$$

where F represents the intensity of the current, E the electromotive power, and $l + \lambda$ the sum of all the resistances of the electric circle. Now if we decompose E into two factors, representing individually the quantity and the tension,

$$F = \frac{q \cdot \frac{1}{q}}{l + \lambda}$$ will be the general formula for the intensity or force of the current, independent of the length of the conducting wire.

The following table will at the same time establish Lenz's law in the case of thermo-electric currents, and prove that even in cases where the tension is so exceedingly low, the elastic force of a given quantity of electricity follows the above-named law.

TABLE L.

	Quantity.	Calculated.	Tension.
Without any wire	720	....	·0930
Interposed w. 1 foot long	298	298	·2080
.. 2 ..	192	192	·3021
.. 3 ..	142	142	·3802
.. 4 ..	112	113	·4375
.. 5 ..	93	93	.4731
.. 6 ..	80	80	·5000

The current here experimented with was generated by a pair of wires of copper and tinned iron $\frac{1}{12}$ inch in diameter, and one foot long, the soldered extremity being immersed in a bath of boiling water, and the free extremity carefully maintained at $59\frac{1}{2}$ Fahr.; the third column in the table being calculated by the aid of the constant 1527.

As respects electricity of high tension, a law extremely analogous to that here indicated may be traced. The striking distance varies directly as the quantity accumulates. If a given jar be successively charged with quantities of electricity, as the numbers 1, 2, 3, 4, &c., the intervals of air through which the spark can pass, vary directly as those numbers. This is abundantly shown by the experiments of Lane, Harris, and other philosophers.

Now upon what does this striking distance depend? Plainly upon the elastic force of the coerced fluid, and therefore the striking distance will measure the elastic force or tension. We condense upon a given surface increasing quantities of the electric fluid, and find that the law in relation to its elastic force, is that the tension of a given quantity is inversely as its volume. But this is the law of Marriotte in relation to the ponderable elastic fluids.

The following numerical determinations were made by adding successive plates to the first single hydro-pair, and taking the values of the current on each addition. It is offered merely as an illustration of the chief fact under discussion, and is not to be regarded as absolutely correct, though every precaution was taken to avoid changes in the current. It shows the decrease of quantity and the increase of tension in Volta's instrument. Of course, in reasoning

upon it, the hypothetical action of each plate is assumed to be equal to that of any other in the series.

TABLE M.

No. of plates.	Quantity.	Tension.
1.	20	·3500
2.	31	·5806
3.	43	·5814
4.	51	·6274
5.	52	·6346

Thermo-electric piles are well known to give the same general results, as respects tension, that hydro-electric piles do: they are much better suited to the purpose of the experimenter, and give currents that are far more constant. The following table represents the action of such a battery, consisting of wires of copper and tinned iron, each element being about one foot long and $\frac{1}{12}$ inch in diameter. The source of heat was a bath of boiling water.

TABLE N.

No. of pairs.	Quantity.	Tension.
1.	256	·1367
2.	305	·2065
3.	325	·2707
4.	348	·3072
5.	352	·3204
11.	396	·5275

The beautiful experiments of Becquerel, and the equally elegant repetition of them by Dr. Golding Bird, show that the view I have here taken of the action of a single pair is correct. The latter chemist found, that not only could a single pair decompose bodies, such as iodide of potassium, &c., which easily yield up their elements, but that the ammoniacal amalgam might be formed, potassium reduced, and in point of fact any decomposition effected. And what is the plan followed? The current is forced to pass, in the electrolyte that is to be decomposed, an obstacle or resisting medium; the tension instantly rises, but at a vast sacrifice of quantity, so that the magnetic needle, which measures only the quantity passing in an indivisible portion of time, is barely affected. Yet, by continuing the current for a great length of time, the resulting decomposing effects are finally the same as those obtained more speedily by the action of many pairs.

How far the experiments given in this memoir bear upon that part of Dr. Faraday's researches in which he has determined the relation of common and voltaic electricity by measure, would form a most important subject of investigation. The results at which he arrives, are in themselves very astonishing, and are fully borne out by his decisive experiments; but when we come to reflect that these results were obtained by the magnetic needle, and electro-chemical action, we may perhaps pause. We may ask, whether it is possible to determine by either of these means, the *absolute* quantity of electricity that passes? Both measure, so to speak, the volume that flows, the one in an indivisible portion of time, the other that which has flowed at the end of a finite time; but do either of them measure the *true absolute* quantity? Can we tell the absolute amount of a gas, without first knowing its condition as to condensation? Can we know *how much* electricity is upon a prime conductor, or compare it with that evolved by a voltaic pile, without first knowing *its* state of condensation? I shall be forgiven for employing these expressions in an unusual way, and for reasoning about this subtile agent as though it were a ponderable body, in as much as it serves, without introducing any hypothesis, to give us more tangible and distinct ideas of what we might otherwise vainly attempt to express.

In the December Number of this Journal, (L. & E. Phil. Mag., vol. xiii. p. 401.) which has just reached me, I find some remarks of Dr. Jacobi on the galvanic spark. Some time ago I came, by another method of experimenting, to the same conclusion. If this spark be really projected by the tension before contact, it ought to take effect at an unlimited distance in a perfect vacuum; but it will be found on making the trial, that if an iron electrode be sealed into the upper part of a barometer tube, and the mercury made to rise gradually towards its point, the spark does not pass until apparent contact takes place: this was found in an analogous but vain attempt to show the thermo-electric spark. It cannot however be entirely, as that philosopher supposes, "simply a phænomenon of combustion," as it is difficult to understand how mercury can enter into combustion in a vacuum*.

Hampden Sydney College, Virginia,
Feb. 22, 1839.

[* May not the spark, in this instance, result from the combination of mercury and iron, under the influence of electricity, and attended by the evolution of heat and light, and thus still be "simply a phænomenon of combustion"? E. W. B.]

LII. *On the Application of Electro-magnetism as a Motive Power: in a Letter from Prof.* P. Forbes *of Aberdeen, to* Michael Faraday, *D.C.L., &c. &c.**

"King's College, Aberdeen, Oct. 7, 1839.

"My dear Sir,

"HAVING seen a notice from Mr. Jacobi sent by you to the London and Edinburgh Philosophical Magazine†, regarding the success of his experiments on the production of a moving power by electro-magnetism, I am sure it will give you pleasure to know that a countryman of our own, Mr. Robert Davidson, of this place, has been eminently successful in his labours in the same field of discovery. For in the first place, he has an arrangement by which with only two electro-magnets and less than one square foot of zinc-surface (the negative metal being copper) a lathe is driven with such velocity as to be capable of turning small articles. Secondly, he has another arrangement, by which, with the same small extent of galvanic power, a small carriage is driven on which two persons were carried along a very coarse wooden floor of a room. And he has a third arrangement, not yet completed, by which, from the imperfect experiments he has made, he expects to gain very considerably more force from the same extent of galvanic power than from either of the other two.

"The first two of these arrangements were seen in operation by Dr. Fleming, Professor of Natural Philosophy in this University, and myself, some days ago; and there remains no doubt on our minds that Mr. Davidson's arrangements will, when finished, be found available as a highly useful, efficient, and exceedingly simple moving power. He has been busily employed for the last two years in his attempts to perfect his machines, during all which time I have been acquainted with his progress, and can bear testimony to the great ingenuity he has shown in overcoming the numberless difficulties he has had to encounter. So far as I know, he was the first who employed the electro-magnetic power in producing motion by simply suspending the magnetism without a change of the poles. This he accomplished about two years ago. About the same time he also constructed galvanic batteries on Professor Daniell's plan by substituting a particular sort of canvas instead of gut, which substitution answers perfectly, is very durable, and can be made of any form or size. And lastly, he has ascertained the kind of iron, and the mode of working it into the best state for producing the strongest magnets with certainty.

"The first two machines, seen in operation by Dr. Fleming and myself, are exceedingly simple, without indeed the least

* Communicated by Dr. Faraday. † See our number for Sept., p. 164.

complexity, and therefore easily manageable, and not liable to derangement. They also take up very little room. As yet the extent of power of which they are capable has not been at all ascertained, as the size of battery employed is so trifling and the magnets so few: but from what can be judged by what is already done, it seems to be probable that a very great power, in no degree even inferior to that of steam, but much more manageable, much less expensive, and occupying greatly less space, if the coals be taken into account, may be obtained.

"In short, the inventions of Mr. Davidson seem to be so interesting to rail-road proprietors in particular, that it would be much for their interest to take up the subject, and be at the expense of making the experiments necessary to bring this power into operation on the great scale, which indeed would be very trifling to a company, while it is very serious for an individual by no means rich, and who has already expended so much of his time and money from the mere desire of perfecting machines which he expected would be so beneficial to his country and to mankind. For it deserves to be mentioned that he has made no secret of his operations, but has shown and explained all that he has done to every one who wished it. His motives have been quite disinterested, and I shall deem it a reproach to our country and countrymen if he is allowed to languish in obscurity, and not have an opportunity afforded him of perfecting his inventions and bringing them into operation, when they promise to be productive of such incalculable advantages*.

"I am, my dear Sir, yours, &c.

"*Michael Faraday, D.C.L., &c. &c.*" "PAT. FORBES."

LIII. *On the Wave-surface in the Theory of Double Refraction.* *By* J. W. LUBBOCK, *Esq., F.R.S.*†

IN the L. and E. Phil. Mag. for November 1837, (vol. xi., p. 417) I endeavoured to present in a concise form the

* [We would observe on the subject of the above communication, that as the late Dr. Schulthess (1833), Prof. Botto (1834), and Dr. Jacobi (1835), whose papers on the application of electricity as a mechanical power have been given in Taylor's Scientific Memoirs, vol. i. p. 503, &c. and vol. ii. p. 1, have given full details of the means they employed, the results obtained, and the cost of the apparatus, it is to be desired that we should be furnished with similar information regarding Mr. Davidson's invention, in order that impartial persons may be enabled to form a judgment on the relative merits of each. A paper by the Rev. J. W. M'Gauley, on the application of magnetism as a moving power, read before the British Association in 1835, will be found in L. & E. Phil. Mag. vol. vii. p. 306, and another, communicated in the following year, in the "Sixth Report" of the Association Transactions of the Sections, p. 24. EDIT.]

† Communicated by the Author.

reasoning of Fresnel, by which the equation to the wave-surface in the theory of double refraction was first established, and to facilitate the comparison of Fresnel's ideas with those which have been since developed by M. Cauchy and by other philosophers.

As Prof. Powell has recently referred to this subject in a communication read at the last meeting of the British Association, I wish to offer some remarks with respect to the existence of *axes of elasticity**, by which I mean axes such that when they are taken for the direction of the coordinate axes x, y, z, the equations of motion of the etherial molecule $\mathfrak{m}$ are of the form

$$\frac{d^2 \xi}{d t^2} = \mathfrak{m} \, \Sigma \, \{\phi \, r + \psi \, (r) \, \Delta \, x^2\} \, \Delta \, \xi$$

(the notation being the same as in my former paper) or in the words of Fresnel, which is the same thing, upon the existence of " trois directions rectangulaires, suivant lesquelles, tout petit déplacement de ce point, en changeant un peu les forces auxquelles il est soumis, produit une résultante totale dirigée dans la ligne même de son déplacement." I conceive that the reasoning of M. Cauchy in the *Nouveaux Exercises*, p. 11, is sufficient to show that such axes generally exist at any point, although I do not recollect that he has anywhere enunciated the proposition precisely in the same shape as Fresnel. This theorem bears a remarkable analogy to that of the existence of three principal axes of rotation in *Mechanics*; and to the other theorem, also well known, that by a proper choice of coordinates, the general equation to the curve surface of the second order in x, y, and z may always be so reduced as not to contain the products $x\,y$, $x\,z$, $y\,z$ multiplied by constant coefficients. Demonstrations of these theorems have been often given, and may be found in the *Mémoire sur l'attraction d'un Ellipsoide homogène*, by M. Poisson, and in his *Mémoire sur le mouvement d'un corps solide.* The same proof *mutatis mutandis* is applicable to both those questions, and also to that of the existence of *axes of elasticity*.

Suppose

$$\Delta \, \xi = \Delta \, \rho \cos X \qquad \Delta \, \eta = \Delta \, \rho \cos Y \qquad \Delta \, \zeta = \Delta \, \rho \cos Z$$

$$\rho = A \cos \, (n \, t - k \, r)$$

$$\Delta \, \rho = - \, 2 \, A \sin^2 \left(\frac{k \, \Delta \, r}{2}\right) \cos \, (n \, t - k \, r)$$
$$+ \, A \sin \, (k \, \Delta \, r) \, \sin \, (n \, t - k \, r).$$

In order to prove the existence of *axes of elasticity*, it is

* See Fresnel's *Mémoire sur la Double Réfraction*, p. 94.

necessary to show that the coordinate axes may always be so assumed that the products

$$\mathfrak{m}\,\Sigma\,\{\psi(r)\,\Delta y\,\Delta x\,\Delta \eta\} \text{ \&c.}$$

are equal to zero.

$$\mathfrak{m}\,\Sigma\,\{\psi(r)\,\Delta y\,\Delta x\,\Delta \eta\} =$$

$$-2\,\mathfrak{m}\,A\cos Y\cos(n\,t-k\,r)\,\Sigma\,\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta x\,\Delta y\}$$

$$+\,\mathfrak{m}\,A\cos Y\sin(n\,t-k\,r)\,\Sigma\,\{\psi(r)\sin(k\,\Delta r)\,\Delta x\,\Delta y\}.$$

If the medium is constituted so that the molecules are distributed symmetrically, as supposed by M. Cauchy, "Si les masses des molécules m, m', m'', &c. sont deux à deux égales entre elles, et distribuées symmetriquement de part et d'autre d'une molécule quelconque $\mathfrak{m}$ sur des droites menées par le point avec lequel cette molécule coincide," (*Nouveaux Exercises*, p. 10.*)

$$\Sigma\,\{\alpha\,\psi\,r\sin(k\,\Delta r)\,\Delta x\,\Delta y\} = 0$$

as is obvious without any proof, and it is only necessary to show that

$$\left.\begin{aligned}
\Sigma\left\{\psi\,r\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta x\,\Delta y\right\} &= 0\\
\Sigma\left\{\psi\,r\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta x\,\Delta z\right\} &= 0\\
\Sigma\left\{\psi\,r\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta y\,\Delta z\right\} &= 0
\end{aligned}\right\} \qquad \text{(A.)}$$

Let

$$\begin{aligned}
x &= a x' + b y' + c z' & \Delta x &= a\,\Delta x' + b\,\Delta y' + c\,\Delta z'\\
y &= a' x' + b' y' + c' z' & \Delta y &= a'\Delta x' + b'\Delta y' + c'\Delta z'\\
z &= a'' x' + b'' y' + c'' z' & \Delta z &= a''\Delta x' + b''\Delta y' + c''\Delta z'.\\
a^2 + b^2 + c^2 &= 1 & a'^2 + b'^2 + c'^2 &= 1\\
a''^2 + b''^2 + c''^2 &= 1 & a'\,a'' + b'\,b'' + c'\,c'' &= 0 \qquad \text{(C.)}\\
a\,a'' + b\,b'' + c\,c'' &= 0 & a\,a' + b\,b' + c\,c' &= 0.
\end{aligned}$$

Let

$$\Sigma\left\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta x'^2\right\} = \text{D}$$

$$\Sigma\left\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta y'^2\right\} = \text{E}$$

$$\Sigma\left\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta z'^2\right\} = \text{F}$$

$$\Sigma\left\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta y'\,\Delta z'\right\} = \text{D}'$$

$$\Sigma\left\{\psi(r)\sin^2\left(\frac{k\,\Delta r}{2}\right)\Delta x'\,\Delta z'\right\} = \text{E}'$$

* Mr. Tovey makes use of the same assumption, Lond. and Edinb. Phil. Mag., 1836, vol. viii. p. 10. and p. 270. This condition will not generally hold at the confines of any medium.

$$\Sigma \left\{ \psi(r) \sin^2 \left(\frac{k \Delta r}{2}\right) \Delta x' \Delta y' \right\} = \text{F}'.$$

The question is to show that the equations (B) and (C) may be satisfied simultaneously by real values of the nine quantities a, b, &c., and to determine them. The proof of this proposition is now, in consequence of the signification which I have here assigned to the quantities D, E, F, D′, E′, F′, word for word the same as that which M. Poisson has given of the existence of three principal axes of rotation in the *Mémoires de l'Académie*, vol. xiv. p. 320, and which of course it is undesirable here to repeat.

The kind of motion here supposed to take place, the etherial molecule vibrating in a straight line in a plane touching the wave-surface, and making with the coordinate axes the constant angles X, Y, Z, is that, contemplated by Fresnel in the reasoning which led him to the equation of the wave-surface.

Suppose the axis x to coincide in direction with the ray, and the vibrations to be performed in planes at right angles to that direction, then $\Delta \xi = 0$; and suppose

$$\eta = \Sigma \{ \alpha \sin (n t - k x) \}$$
$$\zeta = \Sigma \{ \beta \sin (n t - k x + b) \}$$

$$\Delta \eta = \Sigma \left\{ \alpha \left[2 \sin^2 \left(\frac{k \Delta x}{2}\right) \sin (n t - k x) - \sin (k \Delta x) \cos (n t - k x) \right] \right\}$$

$$\Delta \xi = \Sigma \left\{ \beta \left[2 \sin^2 \left(\frac{k \Delta x}{2}\right) \sin (n t - k x + b) - \sin (k \Delta x) \cos (n t - k x + b) \right] \right\}$$

$$\Sigma \left\{ \psi r \Delta z \Delta y \Delta \eta \right\}$$
$$= \Sigma \left\{ \alpha \psi r 2 \sin^2 \left(\frac{k \Delta x}{2}\right) \sin (n t - k x) \Delta z \Delta y \right\}$$
$$- \alpha \psi r \sin (k \Delta x) \cos (n t - k x) \Delta z \Delta y \Big\}$$
$$= \sin (n t - k x) \Sigma \left\{ \alpha \psi (r) 2 \sin^2 \left(\frac{k \Delta x}{2}\right) \Delta z \Delta y \right\}$$
$$- \cos (n t - k x) \Sigma \{ \alpha \psi (r) \sin (k \Delta x) \Delta z \Delta y \}.$$

If the medium be constituted as before, p. 353, line 8, the second term obviously equals zero.

If $y = y' \cos \varepsilon - z \sin \varepsilon \qquad z = z' \cos \varepsilon + y' \sin \varepsilon$
$y' = y \cos \varepsilon + z \sin \varepsilon \qquad z' = z \cos \varepsilon - y \sin \varepsilon.$

The quantity $\Sigma \left\{ \alpha \psi (r) 2 \sin^2 \frac{k \Delta x}{2} \Delta z \Delta y \right\} = 0$, if the

angle ς be so assumed that

$$\tan 2\varsigma = \frac{2\,\Sigma\left\{\alpha\,\psi(r)\,2\sin^2\left(\frac{k\,\Delta\,x}{2}\right)\Delta\,z'\,\Delta\,y'\right\}}{\Sigma\left\{\alpha\,\psi(r)\,2\sin^2\left(\frac{k\Delta x}{2}\right)\Delta\,y'^2\right\} - \Sigma\left\{\alpha\,\psi(r)\,2\sin^2\left(\frac{k\Delta x}{2}\right)\Delta\,z'^2\right\}}$$

The same values will also render equal to zero the quantity

$$\Sigma\,\{\psi(r)\,\Delta\,y\,\Delta\,z\,\Delta\,\zeta\}.$$

The equation

$$(v^2-b^2)\,(v^2-c^2)\,l^2+(v^2-a^2)\,(v^2-c^2)\,m^2 + (v^2-b^2)\,(v^2-a^2)\,n^2 = 0,$$

from which Fresnel deduces the equation to the wave-surface, may be obtained without employing the method of Fresnel, by which it is shown incidentally that the quantity v^2 is a maximum or minimum. Fresnel supposes the constitution of the medium to be such, that when the force is resolved into two components,

1. In the direction of the displacement,
2. In a direction perpendicular to that of the displacement, the second component is also perpendicular to the plane of the wave: the direction of the displacement remains unaltered.

Let $l\,x + m y + n\,z = 0$ be the equation to a plane parallel to the plane of the wave, l, m and n denoting the same quantities as in Mr. Smith's paper in the Cambridge Transactions, vol. vi. p. 87, and in Mr. Sylvester's paper in the Lond. and Edinb. Phil. Mag., 1837, vol. xi. p. 463.

As this plane is also parallel to the direction of displacement,

$$l\cos X + m\cos Y + n\cos Z = 0$$

$$\cos^2 X + \cos^2 Y + \cos^2 Z = 1$$

$$v^2 = a^2\cos^2 X + b^2\cos^2 Y + c^2\cos^2 Z.$$

If $\quad \surd\,\{a^4\cos^2 X + b^4\cos^2 Y + c^4\cos^2 Z\} = f$

the cosines of the angles which the resultant or line of force makes with the axes are

$$-\frac{a^2\cos X}{f}, \qquad -\frac{b^2\cos Y}{f}, \qquad -\frac{c^2\cos Z}{f}$$

if the resultant is contained in a plane parallel to the plane

$$l'\,x + m'\,y + n'\,z = 0$$

$$a^2\,l'\cos X + b^2\,m'\cos Y + c^2\,n'\cos Z = 0.$$

If this plane is also parallel to the direction of displacement,

$$l'\cos X + m'\cos Y + n'\cos Z = 0$$

$$\frac{m'}{l'} = -\frac{(a^2-c^2)\cos X}{(b^2-c^2)\cos Y} \qquad \frac{n'}{l'} = -\frac{(a^2-b^2)\cos X}{(c^2-b^2)\cos Z}.$$

If the planes,

$$l'x + m'y + n'z = 0$$

which is parallel both to the resultant and to the direction of displacement, and

$l\,x + m\,y + n\,z = 0$, which is parallel to the plane of the waves, are perpendicular to each other,

$$l\,l' + m\,m' + n\,n' = 0$$

$$1 + \frac{(a^2-c^2)\cos X\,m}{(c^2-b^2)\cos Y\,l} + \frac{(a^2-b^2)\cos X\,n}{(b^2-c^2)\cos Z\,l} = 0.$$

or, $$(a^2-c^2)\,m\cos X\cos Z + (b^2-a^2)\,n\cos X\cos Y + (c^2-b^2)\,l\cos Y\cos Z = 0.$$

This is the equation of Fresnel, *Mém. de l'Acad.*, vol. vii. p. 115. line 3, and so far the reasoning is the same as his, the only change being in the notation, which I have rendered symmetrical by introducing the quantities l, m, n instead of 1, B, C.

This equation may be put in the form

$$a^2\cos X\{m\cos Z - n\cos Y\} + b^2\cos Y\{n\cos X - l\cos Z\} + c^2\cos Z\{l\cos Y - m\cos X\} = 0 \qquad \text{(D.)}$$

This is the equation (A) of Fresnel, p. 113, and is the same as Mr. Sylvester's equation (6.) L. and E. Phil. Mag., 1837, vol. xi. p. 463. $m\cos Z - n\cos Y$ is identically equal to

$$\frac{\cos Z\{m\cos Z - n\cos Y\}}{\cos Z}$$

$$= \frac{m\{1 - \cos^2 X - \cos^2 Y\} + l\cos X\cos Y + m\cos^2 X.}{\cos Y}.$$

Similarly

$$n\cos X - l\cos Z = \frac{-m\cos X\cos Y - l\{1 - \cos^2 Y\}}{\cos Z}.$$

Substituting these values of $m\cos l - n\cos Y$ and $n\cos X - l\cos Z$ in equation (D.), I get

$$a^2\cos X\{m(1-\cos^2 X) + l\cos X\cos Y\}$$
$$- b^2\cos Y\{l(1-\cos^2 Y) + m\cos X\cos Y\}$$
$$+ c^2\cos Z\{l\cos Y - m\cos X\} = 0.$$

This equation may be put in the form

$$-\{a^2\cos^2 X + b^2\cos^2 Y + c^2\cos^2 Z\}\{m\cos X - l\cos Y\} + a^2\,m\cos X - b^2\,l\cos Y = 0.$$

This equation is identical with the following equation of Fresnel, obtained by differentiations,

$$v^2(\alpha\,n - \beta\,m) = a^2\,\alpha\,n - b^2\,\beta\,m, \text{ p. 130, or}$$
$$v^2\{m\cos X - l\cos Y\} + a^2\,m\cos X - b^2\,l\cos Y = 0,$$

or, $$(v^2-a^2)\, m \cos X = (v^2-b^2)\, l \cos Y$$

$$\frac{\cos X}{\cos Y} = \frac{(v^2-b^2)\, l}{(v^2-a^2)\, m}.$$

Similarly, by symmetry,

$$\frac{\cos Z}{\cos Y} = \frac{(v^2-b^2)\, m}{(v^2-c^2)\, n}.$$

Also, $l \cos X + m \cos Y + n \cos Z = 0$; therefore,

$$l\,\frac{\cos X}{\cos Y} + m + n\,\frac{\cos Z}{\cos Y} = 0$$

$$\frac{\cos Z}{\cos Y} = -\frac{m}{n} - \frac{l}{n}\,\frac{\cos X}{\cos Y}.$$

Similarly, by symmetry,

$$\frac{\cos Z}{\cos X} = -\frac{l}{n} - \frac{m \cos Y}{n \cos X},$$

$$\frac{(v^2-b^2)\, l^2}{(v^2-a^2)\, m} + m + \frac{(v^2-b^2)\, n^2}{(v^2-c^2)\, m} = 0, \quad \text{or,}$$

$$(v^2-b^2)(v^2-c^2)\, l^2 + (v^2-a^2)(v^2-c^2)\, m^2 + (v^2-b^2)(v^2-a^2)\, n^2 = 0$$

$$\cos X = \frac{\dfrac{l}{(v^2-a^2)}}{\sqrt{\left\{\dfrac{l^2}{(v^2-a^2)^2} + \dfrac{m^2}{(v^2-b^2)^2} + \dfrac{n^2}{(v^2-c^2)^2}\right\}}}.$$

Equation (D) may be put in the form

$$a^2\left\{\frac{m \cos Z}{\cos X} - \frac{n \cos Y}{\cos X}\right\} + \frac{b^2 \cos Y}{\cos X}\left\{n - \frac{l \cos Z}{\cos X}\right\}$$
$$+ \frac{c^2 \cos Z}{\cos X}\left\{\frac{l \cos Y}{\cos X} - m\right\} = 0$$

$$\frac{\cos Z}{\cos X} = -\frac{l}{n} - \frac{m \cos Y}{n \cos X}.$$

Hence

$$\frac{\cos^2 Y}{\cos^2 X}\left\{b^2-c^2\right\} + \frac{1}{l\,m}\left\{(b^2-c^2)\, l^2 + (c^2-a^2)\, m^2\right.$$
$$\left. + (b^2-a^2)\, n^2\right\}\frac{\cos Y}{\cos X} + c^2 - a^2 = 0.$$

This is the equation given by Mr. Sylvester in his analytical development of Fresnel's theory, Lond. and Edinb. Phil. Mag., 1837, vol. xi. p. 464.

In the Lond. and Edinb. Phil. Mag. for August 1838, vol. xiii. p. 83. I gave a proof of the property of the conic sec-

tions, that "if any hexagon be circumscribed about any conic section, and the opposite angles be joined, the three diagonals have a common intersection." I believe mine was the first attempt to prove this very curious proposition algebraically, but a simpler demonstration of that theorem has since been given by an anonymous writer in the Cambridge Mathematical Journal, and it follows from the method there adopted that the property which I had noticed for the parabola,

$$y_1 - y_2 + y_3 - y_4 + y_5 - y_6 = 0$$

may be extended to any polygon of an even number of sides circumscribing a parabola, so that

$$y_1 - y_2 + y_3 - y_4 \quad \cdots\cdots\cdots \quad + y_{2n-1} - y_{2n} = 0.$$

LIV. *Experimental Researches in Electricity.—Fifteenth Series.* By MICHAEL FARADAY, *Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acad. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, Gottingen, Modena, Stockholm, &c. &c.**

§ 23. *Notice of the character and direction of the electric force of the Gymnotus.*

1749. WONDERFUL as are the laws and phænomena of electricity when made evident to us in inorganic or dead matter, their interest can bear scarcely any comparison with that which attaches to the same force when connected with the nervous system and with life; and though the obscurity which for the present surrounds the subject may for the time also veil its importance, every advance in our knowledge of this mighty power in relation to inert things, helps to dissipate that obscurity, and to set forth more prominently the surpassing interest of this very high branch of Physical philosophy. We are indeed but upon the threshold of what we may, without presumption, believe man is permitted to know of this matter; and the many eminent philosophers who have assisted in making this subject known, have, as is very evident in their writings, felt up to the latest moment that such is the case.

1750. The existence of animals able to give the same concussion to the living system as the electrical machine, the voltaic battery, and the thunder storm, being with their habits made known to us by Richer, S'Gravesende, Firmin, Walsh, Humboldt, &c. &c., it became of growing importance to identify the living power which they possess, with that which man can call into action from inert matter, and by him named electricity (265, 351.). With the *Torpedo* this has been done to perfection, and the direction of the current of force deter-

* From the Philosophical Transactions for 1839, Part I. p. 1.

mined by the united and successive labours of Walsh*, Cavendish †, Galvani ‡, Gardini §, Humboldt and Gay-Lussac ||, Todd ¶, Sir Humphry Davy**, Dr. Davy††, Becquerel ‡‡, and Matteucci §§.

1751. The Gymnotus has also been experimented with for the same purpose, and the investigations of Williamson || ||, Garden ¶ ¶, Humboldt***, Fahlberg††† and Guisan‡‡‡, have gone very far in showing the identity of the electric force in this animal with the electricity excited by ordinary means; and the two latter philosophers have even obtained the spark.

1752. As an animal fitted for the further investigation of this refined branch of science, the Gymnotus seems, in certain respects, better adapted than the Torpedo, especially (as Humboldt has remarked) in its power of bearing confinement, and capability of being preserved alive and in health for a long period. A Gymnotus has been kept for several months in activity, whereas Dr. Davy could not preserve Torpedos above twelve or fifteen days; and Matteucci was not able out of 116 such fish to keep one living above three days, though every circumstance favourable to their preservation was attended to || || ||. To obtain Gymnoti has therefore been a matter of consequence; and being stimulated, as much as I was honoured, by very kind communications from Baron Humboldt, I in the year 1835 applied to the Colonial Office, where I was promised every assistance in procuring some of these fishes, and continually expect to receive either news of them or the animals themselves.

1753. Since that time Sir Everard Home has also moved a friend to send some Gymnoti over, which are to be consigned to His Royal Highness our late President; and other gentlemen are also engaged in the same work. This spirit induces me to insert in the present communication that part of the letter from Baron Humboldt which I received as an answer to my inquiry of how they were best to be conveyed across the Atlantic. He says, " The Gymnotus, which is

* Philosophical Transactions, 1773, p. 461. † Ibid. 1776, p. 196.

‡ Aldini's Essai sur le Galvanisme, ii. 61.

§ De Electrici Ignis Natura, §. 71. Mantua, 1792.

|| Annales de Chimie, xiv. 15. ¶ Phil. Trans., 1816, p. 120. [or Phil. Mag., First Series, vol. xlviii. p. 14.] ** Ibid. 1829, p. 15. [or Phil. Mag. and Annals, vol. vi. p. 81.] †† Ibid. 1832, p. 259; and 1834, p. 531. [see Lond. and Edinb. Phil. Mag., vol. i. p. 67; xi. p. 57.]

‡‡ Traité de l'Electricité, iv. 264. §§ Bibliothèque Universelle, 1837, tom. xii. 163. [see Lond. and Edinb. Phil. Mag., vol. xii. p. 196.]

|| || Phil. Trans., 1775, p. 94. ¶ ¶ Ibid. 1775, p. 102.

*** Personal Narrative, chap. xvii. ††† Swedish Transactions, 1801, pp. 122. 156. ‡‡‡ De Gymnoto Electrico. Tubingen, 1819.

|| || || Bibliothèque Universelle, 1837, xii. p. 174.

common in the Llanos de Caracas (near Calabozo), in all the small rivers which flow into the Orinoco, in English, French or Dutch Guiana, is not of difficult transportation. We lost them so soon at Paris because they were too much fatigued (by experiments) immediately after their arrival. MM. Norderling and Fahlberg retained them alive at Paris above four months. I would advise that they be transported from Surinam (from Essequibo, Demerara, Cayenne) in summer, for the Gymnotus in its native country lives in water of 25° centigrade (or 77° Fahr.). Some are five feet in length, but I would advise that such as are about twenty-seven or twenty-eight inches in length be chosen. Their power varies with their food, and their state of rest. Having but a small stomach they eat little and often, their food being cooked meat, *not salted*, small fish, or even bread. Trial should be made of their strength and the fit kind of nourishment before they are shipped, and those fish only selected already accustomed to their prison. I retained them in a box or trough about four feet long, and sixteen inches wide and deep. The water must be *fresh*, and be changed every three or four days: the fish must not be prevented from coming to the surface, for they like to swallow air. A net should be put over and round the trough, for the Gymnotus often springs out of the water. These are all the directions that I can give you. It is, however, *important* that the animal should not be tormented or fatigued, for it becomes exhausted by frequent electric explosions. Several Gymnoti may be retained in the same trough."

1754. A Gymnotus has lately been brought to this country by Mr. Porter, and purchased by the proprietors of the Gallery in Adelaide Street; they immediately most liberally offered me the liberty of experimenting with the fish for scientific purposes; they placed it for the time exclusively at my disposal, that (in accordance with Humboldt's directions (1753.)) its powers might not be impaired; only desiring me to have a regard for its life and health. I was not slow to take advantage of their wish to forward the interests of science, and with many thanks accepted their offer. With this Gymnotus, having the kind assistance of Mr. Bradley of the Gallery, Mr. Gassiot, and occasionally other gentlemen, as Professors Daniell, Owen, and Wheatstone, I have obtained every proof of the identity of its power with common electricity (265. 351, &c.). All of these had been obtained before with the Torpedo (1750.), and some, as the shock, circuit, and spark (1751.), with the Gymnotus; but still I think a brief account of the results will be acceptable to the Royal So-

ciety, and I give them as necessary preliminary experiments to the investigations which we may hope to institute when the expected supply of animals arrives (1752.).

1755. The fish is forty inches long. It was caught about March 1838; was brought to the Gallery on the 15th of August, but did not feed from the time of its capture up to the 19th of October. From the 24th of August Mr. Bradley nightly put some blood into the water, which was changed for fresh water next morning, and in this way the animal perhaps obtained some nourishment. On the 19th of October it killed and eat four small fish; since then the blood has been discontinued, and the animal has been improving ever since, consuming upon an average one fish daily*.

1756. I first experimented with it on the 3rd of September, when it was apparently languid, but gave strong shocks when the hands were favourably disposed on the body (1760. 1773, &c.). The experiments were made on four different days, allowing periods of rest from a month to a week between each. His health seemed to improve continually, and it was during this period, between the third and fourth days of experiment, that he began to eat.

1757. Beside the hands two kinds of collectors were used. The one sort consisted each of a copper rod fifteen inches long, having a copper disc one inch and a half in diameter brazed to one extremity, and a copper cylinder to serve as a handle, with large contact to the hand, fixed to the other, the rod from the disc upwards being well covered with a thick caoutchouc tube to insulate that part from the water. By these the states of particular parts of the fish whilst in the water could be ascertained.

1758. The other kind of collectors were intended to meet the difficulty presented by the complete immersion of the fish in water; for even when obtaining the spark itself I did not think myself justified in asking for the removal of the animal into air. A plate of copper eight inches long by two inches and a half wide, was bent into a saddle shape, that it might pass over the fish, and inclose a certain extent of the back and sides, and a thick copper wire was brazed to it, to convey the electric force to the experimental apparatus; a jacket of sheet caoutchouc was put over the saddle, the edges projecting at the bottom and the ends; the ends were made to converge so as to fit in some degree the body of the fish, and the bottom edges were made to spring against any horizontal surface on which the saddles were placed. The part of the wire liable to be in the water was covered with caoutchouc.

* The fish eaten were gudgeons, carp, and perch.

1759. These conductors being put over the fish, collected power sufficient to produce many electric effects; but when, as in obtaining the spark, every possible advantage was needful, then glass plates were placed at the bottom of the water, and the fish being over them, the conductors were put over it until the lower caoutchouc edges rested on the glass, so that the part of the animal within the caoutchouc was thus almost as well insulated as if the Gymnotus had been in the air.

1760. *Shock*. The shock of this animal was very powerful when the hands were placed in a favourable position, i. e. one on the body near the head, and the other near the tail; the nearer the hands were together within certain limits the less powerful was the shock. The disc conductors (1757.) conveyed the shock very well when the hands were wetted and applied in close contact with the cylindrical handles; but scarcely at all if the handles were held in the dry hands in an ordinary way.

1761. *Galvanometer*. Using the saddle conductors (1758.) applied to the anterior and posterior parts of the Gymnotus, a galvanometer was readily affected. It was not particularly delicate; for zinc and platina plates on the upper and lower surface of the tongue did not cause a permanent deflection of more than 25°; yet when the fish gave a powerful discharge the deflection was as much as 30°, and in one case even 40°. The deflection was constantly in a given direction, the electric current being always from the anterior parts of the animal through the galvanometer wire to the posterior parts. The former were therefore for the time externally positive, and the latter negative.

1762. *Making a magnet*. When a little helix containing twenty-two feet of silked wire wound on a quill was put into the circuit, and an annealed steel needle placed in the helix, the needle became a magnet, and the direction of its polarity in every case indicated a current from the anterior to the posterior parts of the Gymnotus through the conductors used.

1763. *Chemical decomposition*. Polar decomposition of a solution of iodide of potassium was easily obtained. Three or four folds of paper moistened in the solution (322.) were placed between a platina plate and the end of a wire also of platina, these being respectively connected with the two saddle conductors (1758.). Whenever the wire was in conjunction with the conductor at the fore part of the Gymnotus, iodine appeared at its extremity; but when connected with the other conductor none was evolved at the place on the paper where it before appeared. So that here again the direction of the current proved to be the same as that given by the former tests.

1764. By this test I compared the middle part of the fish with other portions before and behind it, and found that the conductor A, which being applied to the middle was negative to the conductor B applied to the anterior parts, was, on the contrary, positive to it when B was applied to places near the tail. So that within certain limits the condition of the fish externally at the time of the shock appears to be such, that any given part is negative to other parts anterior to it, and positive to such as are behind it.

1765. *Evolution of heat.* Using a Harris's thermo-electrometer belonging to Mr. Gassiot, we thought we were able in one case, namely, that when the deflection of the galvanometer was 40° (1761.), to observe a feeble elevation of temperature. I was not observing the instrument myself, and one of those who at first believed they saw the effect now doubts the result*.

1766. *Spark.* The electric spark was obtained thus. A good magneto-electric coil, with a core of soft iron wire, had one extremity made fast to the end of one of the saddle collectors (1758.), and the other fixed to a new steel file; another file was made fast to the end of the other collector. One person then rubbed the point of one of these files over the face of the other, whilst another person put the collectors over the fish, and endeavoured to excite it to action. By the friction of the files contact was made and broken very frequently; and the object was to catch the moment of the current through the wire and helix, and by breaking contact *during the current* to make the electricity sensible as a spark.

1767. The spark was obtained four times, and nearly all who were present saw it. That it was not due to the mere attrition of the two piles was shown by its not occurring when the files were rubbed together, independently of the animal. Since then I have substituted for the lower file a revolving steel plate, cut file-fashion on its face, and for the upper file wires of iron, copper and silver, with all of which the spark was obtained †.

1768. Such were the general electric phænomena obtained from this Gymnotus whilst living and active in its native element. On several occasions many of them were obtained together; thus a magnet was made, the galvanometer de-

* In more recent experiments of the same kind we could not obtain the effect.

† At a later meeting, at which attempts were made to cause the attraction of gold leaves, the spark was obtained directly between fixed surfaces, the inductive coil (1766.) being removed, and only short wires (by comparison) employed.

flected, and perhaps a wire heated, by one single discharge of the electric force of the animal.

1769. I think a few further but brief details of experiments relating to the quantity and disposition of the electricity in and about this wonderful animal will not be out of place in this short account of its powers.

1770. When the shock is strong, it is like that of a large Leyden battery charged to a low degree, or that of a good voltaic battery of perhaps one hundred or more pairs of plates, of which the circuit is completed for a moment only. I endeavoured to form some idea of the *quantity* of electricity by connecting a large Leyden battery (291.) with two brass balls, above three inches in diameter, placed seven inches apart in a tub of water, so that they might represent the parts of the Gymnotus to which the collectors had been applied; but to lower the intensity of the discharge, eight inches in length of six-fold thick wetted string were interposed elsewhere in the circuit, this being found necessary to prevent the easy occurrence of the spark at the ends of the collectors (1758.), when they were applied in the water near to the balls, as they had been before to the fish. Being thus arranged, when the battery was strongly charged and discharged, and the hands put into the water near the balls, a shock was felt, much resembling that from the fish; and though the experiments have no pretension to accuracy, yet as the tension could be in some degree imitated by reference to the more or less ready production of a spark, and after that the shock be used to indicate whether the quantity was about the same, I think we may conclude that a single medium discharge of the fish is at least equal to the electricity of a Leyden battery of fifteen jars, containing 3500 square inches of glass coated on both sides, charged to its highest degree (291.). This conclusion respecting the great quantity of electricity in a single Gymnotus shock, is in perfect accordance with the degree of deflection which it can produce in a galvanometer needle (367. 860. 1761.), and also with the amount of chemical decomposition produced (374. 860. 1763.) in the electrolyzing experiments.

1771. Great as is the force in a single discharge, the Gymnotus, as Humboldt describes, and as I have frequently experienced, gives a double and even a triple shock; and this capability of immediately repeating the effect with scarcely a sensible interval of time, is very important in the considerations which must arise hereafter respecting the origin and excitement of the power in the animal. Walsh, Humboldt, Gay-Lussac, and Matteucci have remarked the same thing of the Torpedo, but in a far more striking degree.

1772. As, at the moment when the fish wills the shock, the anterior parts are positive and the posterior parts negative, it may be concluded that there is a current from the former to the latter through every part of the water which surrounds the animal, to a considerable distance from its body. The shock which is felt, therefore, when the hands are in the most favourable position, is the effect of a very small portion only of the electricity which the animal discharges at the moment, by far the largest portion passing through the surrounding water. This enormous external current must be accompanied by some effect within the fish *equivalent* to a current, the direction of which is from the tail towards the head, and equal to the sum of *all these external forces.* Whether the process of evolving or exciting the electricity within the fish includes the production of this internal current (which need not of necessity be as quick and momentary as the external one), we cannot at present say; but at the time of the shock the animal does not apparently feel the electric sensation which he causes in those around him.

1773. By the help of the accompanying diagram I will state a few experimental results which illustrate the current around the fish, and show the cause of the difference in character of the shock occasioned by the various ways in which the person is connected with the animal, or his position altered with respect to it. The large circle represents the tub in which the animal is confined; its diameter is forty-six inches, and the depth of water in it three inches and a half; it is supported on dry wooden legs. The figures represent the places where the hands or the disc conductors (1757.) were applied, and where they are close to the figure of the animal, it implies that contact with the fish was made. I will designate different persons by A, B, C, &c., A being the person who excited the fish to action.

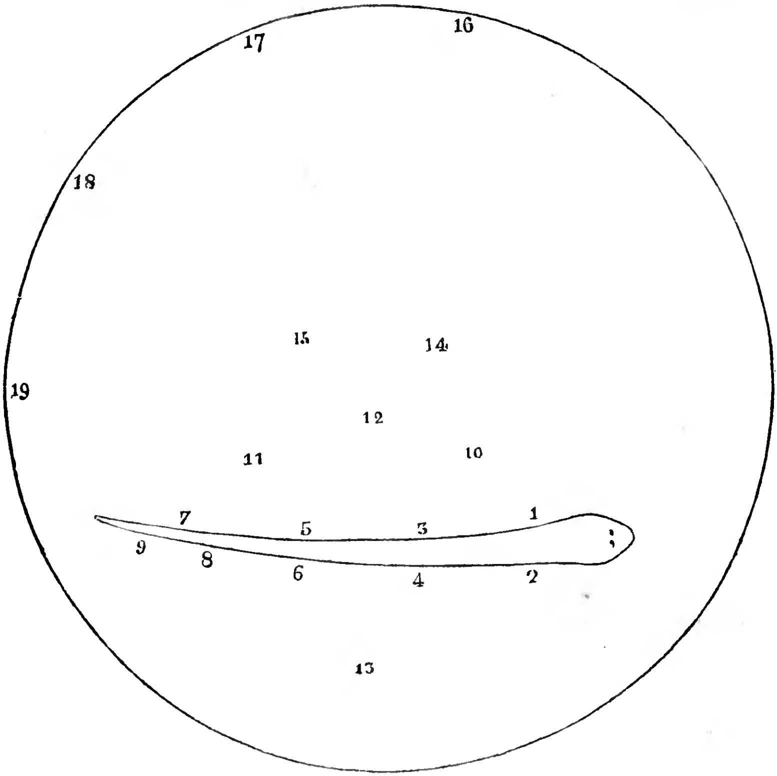
1774. When one hand was in the water the shock was felt in that hand only, whatever part of the fish it was applied to; it was not very strong, and was only in the part immersed in the water. When the hand and part of the arm was in, the shock was felt in all the parts immersed.

1775. When *both* hands were in the water at the *same* part of the fish, still the shock was comparatively weak, and only in the parts immersed. If the hands were on opposite sides, as at 1, 2, or at 3, 4, or 5, 6, or if one was above and the other below at the same part, the effect was the same. When the disc collectors were used in these positions no effect was felt by the person holding them, (and this corresponds with the observation of Gay-Lussac on Torpedos*,) whilst other

* Annales de Chimie, xiv. p. 18.

persons, with both hands in at a distance from the fish, felt considerable shocks.

1776. When both hands or the disc collectors were applied at places separated by a part of the length of the animal, as at 1, 3, or 4, 6, or 3, 6, then strong shocks extending up the arms, and even to the breast of the experimenter, occur-



red, though another person with a single hand in at any of these places, felt comparatively little. The shock could be obtained at parts very near the tail, as at 8, 9. I think it was strongest at about 1 and 8. As the hands were brought nearer together the effect diminished, until being in the same cross plane, it was, as before described, only sensible in the parts immersed (1775.)

1777. B placed his hands at 10, 11, at least four inches from the fish, whilst A touched the animal with a glass rod to excite it to action; B quickly received a powerful shock. In another experiment of a similar kind, as respects the non-

necessity of touching the fish, several persons received shocks independently of each other; thus A was at 4, 6; B at 10, 11; C at 16, 17; and D at 18, 19; all were shocked at once, A and B very strongly, C and D feebly. It is very useful whilst experimenting with the galvanometer or other instrumental arrangements, for one person to keep his hands in the water at a moderate distance from the animal, that he may know and give information when a discharge has taken place.

1778. When B had both hands at 10, 11, or at 14, 15, whilst A had but one hand at 1, or 3, or 6, the former felt a strong shock, whilst the latter had but a weak one, though in contact with the fish. Or if A had both hands in at 1, 2, or 3, 4, or 5, 6, the effect was the same.

1779. If A had the hands at 3, 5, B at 14, 15, and C at 16, 17, A received the most powerful shock, B the next powerful, and C the feeblest.

1780. When A excited the gymnotus by his hands at 8, 9, whilst B was at 10, 11, the latter had a much stronger shock than the former, though the former touched and excited the animal.

1781. A excited the fish by one hand at 3, whilst B had both hands at 10, 11, (or along), and C had the hands at 12, 13 (or across); A had the pricking shock in the immersed hand only (1774.); B had a strong shock up the arms; C felt but a slight effect in the immersed parts.

1782. The experiments I have just described are of such a nature as to require many repetitions before the general results drawn from them can be considered as established; nor do I pretend to say that they are anything more than indications of the direction of the force. It is not at all impossible that the fish may have the power of throwing each of its four electric organs separately into action, and so to a certain degree direct the shock, i. e. he may have the capability of causing the electric current to emanate from one side, and at the same time bring the other side of his body into such a condition, that it shall be as a non-conductor in that direction. But I think the appearances and results are such as to forbid the supposition, that he has any control over the direction of the currents after they have entered the fluid and substances around him.

1783. The statements also have reference to the fish when in a straight form; if it assume a bent shape, then the lines of force around it vary in their intensity in a manner that may be anticipated theoretically. Thus if the hands were applied at 1, 7, a feebler shock in the arms would be expected if the animal were curved with that side inwards, than if it were straight, because the distance between the parts

would be diminished, and the intervening water therefore conduct more of the force. But with respect to the parts *immersed*, or to animals, as fish *in the water* between 1 and 7, they would be more powerfully, instead of less powerfully, shocked.

1784. It is evident from all the experiments, as well as from simple considerations, that all the water and all the conducting matter around the fish through which a discharge circuit can in any way be completed, is filled at the moment with circulating electric power; and this state might be easily represented generally in a diagram by drawing the lines of inductive action (1231. 1304. 1338.) upon it: in the case of a gymnotus, surrounded equally in all directions by water, these would resemble generally, in disposition, the magnetic curves of a magnet, having the same straight or curved shape as the animal, i. e. provided he, in such cases, employed, as may be expected, his four electric organs at once.

1785. This gymnotus can stun and kill fish which are in very various positions to its own body; but on one day when I saw it eat, its action seemed to me to be peculiar. A live fish about five inches in length, caught not half a minute before, was dropped into the tub. The gymnotus instantly turned round in such a manner as to form a coil inclosing the fish, the latter representing a diameter across it; a shock passed, and there in an instant was the fish struck motionless, as if by lightning, in the midst of the waters, its side floating to the light. The gymnotus made a turn or two to look for its prey, which having found he bolted, and then went searching about for more. A second smaller fish was given him, which being hurt in the conveyance, showed but little signs of life, and this he swallowed at once, apparently without shocking it. The coiling of the gymnotus round its prey had, in this case, every appearance of being intentional on its part, to increase the force of the shock, and the action is evidently exceedingly well suited for that purpose (1783.), being in full accordance with the well-known laws of the discharge of currents in masses of conducting matter; and though the fish may not always put this artifice in practice, it is very probable he is aware of its advantage, and may resort to it in cases of need.

1786. Living as this animal does in the midst of such a good conductor as water, the first thoughts are thoughts of surprise that it can sensibly electrify anything; but a little consideration soon makes one conscious of many points of great beauty, illustrating the wisdom of the whole arrange-

ment. Thus the very conducting power which the water has; that which it gives to the moistened skin of the fish or animal to be struck; the extent of surface by which the fish and the water conducting the charge to it are in contact; all conduce to favour and increase the shock upon the doomed animal, and are in the most perfect contrast with the inefficient state of things which would exist if the gymnotus and the fish were surrounded by air; and at the same time that the power is one of low intensity, so that a dry skin wards it off, though a moist one conducts it (1760.): so is it one of great quantity (1770.), that though the surrounding water does conduct away much, enough to produce a full effect may take its course through the body of the fish that is to be caught for food, or the enemy that is to be conquered.

1787. Another remarkable result of the relation of the gymnotus and its prey to the medium around them is, that the larger the fish to be killed or stunned, the greater will be the shock to which it is subject, though the gymnotus may exert only an equal power; for the large fish has passing through its body those currents of electricity, which, in the case of a smaller one, would have been conveyed harmless by the water at its sides.

1788. The gymnotus appears to be sensible when he has shocked an animal, being made conscious of it, probably, by the *mechanical impulse* he receives, caused by the spasms into which it is thrown. When I touched him with my hands, he gave me shock after shock; but when I touched him with glass rods, or the insulated conductors, he gave one or two shocks, felt by others having their hands in at a distance, but then ceased to exert the influence, as if made aware it had not the desired effect. Again, when he has been touched with the conductors several times, for experiments on the galvanometer or other apparatus, and appears to be languid or indifferent, and not willing to give shocks, yet being touched by the hands, they, by convulsive motion, have informed him that a sensitive thing was present, and he has quickly shown his power and his willingness to astonish the experimenter.

1789. It has been remarked by Geoffroy St. Hilaire, that the electric organs of the Torpedo, Gymnotus, and similar fishes, cannot be considered as essentially connected with those which are of high and direct importance to the life of the animal, but to belong rather to the common teguments; and it has also been found that such Torpedos as have been deprived of the use of their peculiar organs, have continued the functions of life quite as well as those in which

they were allowed to remain. These, with other considerations, lead me to look at these parts with a hope that they may upon close investigation prove to be a species of natural apparatus, by means of which we may apply the principles of *action and re-action* in the investigation of the nature of the *nervous influence.*

1790. The anatomical relation of the nervous system to the electric organ; the evident exhaustion of the nervous energy during the production of electricity in that organ; the apparently equivalent production of electricity in proportion to the quantity of nervous force consumed; the constant direction of the current produced, with its relation to what we may believe to be an equally constant direction of the nervous energy thrown into action at the same time; all induce me to believe, that it is not impossible but that, on passing electricity per force through the organ, a reaction back upon the nervous system belonging to it might take place, and that a restoration, to a greater or smaller degree, of that which the animal expends in the act of exciting a current, might perhaps be effected. We have the analogy in relation to heat and magnetism. Seebeck taught us how to commute heat into electricity; and Peltier has more lately given us the strict converse of this, and shown us how to convert the electricity into heat, including both its relation of hot and cold. Oersted showed how we were to convert electric into magnetic forces, and I had the delight of adding the other member of the full relation, by reacting back again and converting magnetic into electric forces. So perhaps in these organs, where nature has provided the apparatus by means of which the animal can exert and convert nervous into electric force, we may be able, possessing in that point of view a power far beyond that of the fish itself, to re-convert the electric into the nervous force.

1791. This may seem to some a very wild notion, as assuming that the nervous power is in some degree analogous to such powers as heat, electricity, and magnetism. I am only assuming it, however, as a reason for making certain experiments, which, according as they give positive or negative results, will regulate further expectation. And with respect to the nature of nervous power, that exertion of it which is conveyed along the nerves to the various organs which they excite into action, is not the direct principle of *life*; and therefore I see no natural reason why we should not be allowed in certain cases to *determine* as well as observe its course. Many philosophers think the power is electricity. Priestley put forth this view in 1774 in a very striking and distinct form, both as regards ordinary animals and those

which are electric, like the Torpedo*. Dr. Wilson Philip considers that the agent in certain nerves is electricity modified by vital action†. Matteucci thinks that the nervous fluid or energy, in the nerves belonging to the electric organ at least, is electricity ‡. MM. Prevost and Dumas are of opinion that electricity moves in the nerves belonging to the muscles; and M. Prevost adduces a beautiful experiment, in which steel was magnetized, in proof of this view; which, if it should be confirmed by further observation, and by other philosophers, is of the utmost consequence to the progress of this high branch of knowledge§. Now though I am not as yet convinced by the facts that the nervous fluid is only electricity, still I think that the agent in the nervous system may be an inorganic force; and if there be reasons for supposing that magnetism is a higher relation of force than electricity (1664. 1731. 1734.), so it may well be imagined, that the nervous power may be of a still more exalted character, and yet within the reach of experiment.

1792. The kind of experiment I am bold enough to suggest is as follows. If a Gymnotus or Torpedo has been fatigued by frequent exertion of the electric organs, would the sending of currents of similar force to those he emits, or of other degrees of force, either continuously or intermittingly in the same direction as those he sends forth, restore him his powers and strength more rapidly than if he were left to his natural repose?

1793. Would sending currents through in the contrary direction exhaust the animal rapidly? There is, I think, reason to believe that the Torpedo (and perhaps the Gymnotus) is not much disturbed or excited by electric currents sent only through the electric organ; so that these experiments do not appear very difficult to make.

1794. The disposition of the organs in the Torpedo suggest still further experiments on the same principle. Thus when a current is sent in the natural direction, i. e. from below upwards through the organ on one side of the fish, will it

* Priestley on Air, vol. i. p. 277. Edition of 1774.

† Dr. Wilson Philip is of opinion, that the nerves which excite the muscles and effect the chemical changes of the vital functions, operate by the electric power supplied by the brain and spinal marrow, in its effects, modified by the vital powers of the living animal; because he found, as he informs me, as early as 1815, that while the vital powers remain, all these functions can be as well performed by voltaic electricity after the removal of the nervous influence, as by that influence itself; and in the end of that year he presented a paper to the Royal Society, which was read at one of their meetings, giving an account of the experiments on which this position was founded.

‡ Bibliothèque Universelle, 1837, tom. xii. 192.

§ Ibid., 1837, xii. 202: xiv. 200.

excite the organ on the other side into action? or if sent through in the contrary direction, will it produce the same or any effect on that organ? Will it do so if the nerves proceeding to the organ or organs be tied? and will it do so after the animal has been so far exhausted by previous shocks as to be unable to throw the organ into action in any, or in a similar, degree of his own will?

1795. Such are some of the experiments which the conformation and relation of the electric organs of these fishes suggest, as being rational in their performance, and promising in anticipation. Others may not think of them as I do; but I can only say for myself, that were the means in my power, they are the very first that I would make.

Royal Institution, Nov. 9, 1838.

LV. *Observations of Shooting Stars made on the Night of August* 10, 1839. *By* Edward Cooper, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Birmingham, Aug. 31, 1839.

YOUR insertion of my letter to Dr. Robinson last year, in the London and Edinburgh Philosophical Magazine, giving an account of observations on shooting stars made on the 10th of August at Geneva, induces me to trouble you with the present communication.

I was stationed at Brandsbury House, about three miles N.W. of London, on the night of the last 10th of August, and was assisted in my observations by two friends of mine, Messrs. Jones and Fenton. We confined our range to the Via Lactea, and expanse of sky to the north of it, as I had requested Sir James South, and the Messrs. Gwielt of London, to direct their attention to the south. During the preceding day the wind was pretty fresh from the S.W. and S.S.W. with flying clouds. An almost perfect lull of wind and a clear sky succeeded at about 7^h, and continued till about $11^h\ 45^m$. At about $12^h\ 30^m$ it was cloudy towards the N.W., and 2^m or 3^m later also towards N.E. At $12^h\ 50^m$ all was again clear, and the stars appeared to be set on a perfectly black ground. These favourable circumstances continued until $13^h\ 35^m$, when the wind began to stir, and seemed northing a little, and several clouds of dark hue rose from the N. and N.E. horizon. These however partially passed away, but in 10^m more the N.W. horizon showed symptoms of spreading a more formidable veil. This gradually extended itself, with occasional openings, over the entire heavens, till at $14^h\ 15^m$ all was shut out, and after waiting some time without any change

for the better we retired to rest. The dew was very heavy until 12^h, but after 13^h it entirely disappeared. During 3^h 22^m, actual observation, we noted 152 shooting stars, of which I have registered the times of apparition, the directions, the estimated duration, and magnitudes of 138. Of the remainder merely the time of apparition was taken down, in consequence of the rapid succession of the phænomena, and the impossibility of keeping in mind all their directions, &c. Three or four were very splendid, but none equal to the finest seen on the 10th of August 1838, at Geneva. The general result however fully establishes the fact that the nights of the 10th or 11th of August furnish a most remarkable exhibition of these interesting celestial travellers, and I think that experience fully justifies the prediction that these, and the nights 13th and 14th of November, will in future be their established galas. The average number per hour in the half hemisphere to which we attended was 44; exceeding considerably the average of last year at Geneva; but it should be recollected that the total absence of the moon this year afforded a considerable advantage to observers over the corresponding nights of last year. The only circumstance particularly worthy of notice this year is the fact that several of the shooting stars appeared to move *upwards*, whereas no instance of this was remarked last year at Geneva. I subjoin my form of observing, and a list for this year, similar to that which I sent last year to Dr. Robinson.

Richard Taylor, Esq., Yours, &c.
&c. &c. &c. EDWARD COOPER.

Aug. 10. 9^h 15^m from α Urs. Min. to β Ophiuchi. Duration $1^s{\cdot}5$. Mag. 4. Brandsbury.—No train.

Aug. 10. 9^h 18^m from ϵ Cassiop. to η Coron. Bor. Duration 6. Mag. 1. Brandsbury.—Brilliant train, blue.

1839.

From		To	From		To
0.	Aquila	7.	24.	Draco	11.
0.	Auriga	2.	3.	Hercules	14.
4.	Bootes	24.	9.	Lyra.	8.
0.	Camelop.	2.	1.	Ophiuch.	10.
4.	Can. Ven.	5.	5.	Perseus	3.
19.	Cassiop.	1.	0.	Sagitta.	2.
3.	Cepheus	0.	1.	Serpens.	2.
8.	Cor. Bor.	6.	24.	Urs. Maj.	22.
6.	Cygnus	3.	25.	Urs. Min.	11.
1.	Delphin.	1.	0.	Virgo	2.

Total 137 *from*, 136 *to*. In one instance the place where the phænomena appeared, and in two instances the place where the phænomena disappeared, was not noted.

LVI. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel RICHARD WRIGHT, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 304.]

Ascent from the Coast to the Table Lands of Quito.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1825.				
May 1.	79°	7 a.m.	0	Savaneta.
2.	83	2 p.m.	B. W.	Fair.
3.	79	7 a.m.	212°.	Id.
,,	86·5	2 p.m.		
4.	78	7 a.m.		Rain.
,,	82	2 p.m.		
5.	83	2 p.m.		Showery.
6.	71	7 a.m.	3025 ft.	Ascent of Angas.
,,	75	2 p.m.	B.W.207.	
10.	68	7 a.m.	id.	Tambo de Jorge.
11.	56	7 a.m.	7865 ft.	Camino Real.—Showery.
12.	59	8 a.m.	B.W.199.	San Miguel.
13.	60	8 a.m.	......	Fair.

Town of Guaranda. Lat. 1° 37′ *S. Foot of Chimborazo.*

15.	54	6 a.m.	9075 ft.	Cloudy.
16.	59	2 p.m.	B.W.197.	
17.	56	6 a.m.		Fair.
,,	64	2 p.m.		
18.	54	6 a.m.		Id.
,,	64	2 p.m.		
19.	54	7 a.m		Id.
20, 21.	} 65	2 p.m.		
22.	} 53	7 a.m.		Cloudy.
23.	54	7 a.m.		Clear.
24.	62	2 p.m.		
25.	54	7 a.m.		Cloudy with *Parametos,* or mountain mists. Wind S.
to 28.	60	2 p.m.		
June 1.	58	7 a.m.		Fair.
,,	63	2 p.m.		
2.	56	7 a.m.		Showers in the evening.
,,	63	2 p.m.		
3.	56	7 a.m.		Fair.
,,	65	2 p.m.		

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1825. June 4.	56°	7 a.m.	9075 ft.	Cloudy and misty.
5.	63	2 p.m.		Rain.
12,	56	7 a.m.		Fair.
to 21.	64	2 p.m.		
22.	57	7 a.m.		Fair with wind.
,,	65	2 p.m.		
34 days 63°·07 max. 55 ·15 min. } Med. 59°·11.				

Village of Cimiatuc, on the eastern slope of Chimborazo.

May 30.	55	7 a.m.	11,495 ft.	Fair.
,,	57	4 p.m.	B. W.	
31.	55	7 a.m.	193°.	

Guaranda.

1828. Dec. 6.	58	8 a.m.		Fair.
,,	59	11		
,,	60·5	2 p.m.		
,,	59	5½		
,,	58	9		
Mean 58°·9.				
7.	57	7½ a.m.		Fair. Sun 100° reflected heat.
,,	58	10		
,,	60·5	3 p.m.		
Mean 58°·5.				
8.	56	6 a.m.		Id.
,,	60	2 p.m.		
,,	58·25	mean.		
3 days. Mean 58°·55. Of 37 days 58°·83.				

Ascent from the Table-land of Riobamba to the Mine of Condorasto, on the Cordillera of El Altar.

Date.	Time.	Thermometer.	Remarks.	Elevation.	
1825.					
Sept. 20.	8 a.m.	51°0	Cloudy.	9377 ft.	
21.	2 p.m.	58·0	Wind.	B.W. 196½°	Riobamba.
22.	8 a.m.	51.0	Clear.		
"	2 p.m.	60·0			
26.	8 a.m.	48·0	Fair.	11,797 ft. B.W. 192½°	Rio Blanco.
27.	8 a.m.	42·0	Misty.	13,007 ft. B.W. 190½°.	Los Cherillos.
"	12	45·0	Misty.	14,520 ft. B.W. 188.	Mine of Condorasto.

Town of Llactacunga. Lat. 55′ 14″ *S.*
Table-land of Quito.

Date.	Thermometer.	Time.	Elevation.	Remarks.
1826.				
Jan. 25.	56°0	6 a.m.	10,285 ft.	
26.	61·0	3 p.m.	B. W.	Fair.
27.	56·0	6 a.m.	195°.	Id.
"	60·5	3 p.m.		
28.	56·0	6 a.m.		Rain.
"	62·0	3 p.m.		
29.	57·0	6 a.m.		Fair.
"	61·0	3 p.m.		
30.	60·0	3 p.m.		Cloudy.
31.	54·0	6 a.m.		
"	60·0	3 p.m.		Fair.
Feb. 1.	56·0	6 a.m.		Id.
2.	61·0	3 p.m.		Id.
3, 4, 5.	60·0	2 p.m.		Id.
6, 7, 8.	58·0	2 p.m.		
15 days. Max. 60°·01, Min. 55 ·85 } Med. 57°·93.				

City of Quito. Lat. 13′ 27″ *S.*

1825.				
July 10.	57·0	7 a.m.	9537 ft.	Fair.
11.	61·0	3 p.m.	B. W.	
12.	57·0	7 a.m.	196°¼.	Id.
13.	63·0	3 p.m.		

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1825. July 14.	58°·0	7 a.m.	0	Fair.
„	64·0	3 p.m.		
15.	58·0	7 a.m.		Id.
16.	67·0	3 p.m.		
17.	57·0	7 a.m.		Id.
18.	64.0	3 p.m.		
19.	60·0	7 a.m.		Id.
20,	67·0	3 p.m.		
21.	60·0	7 a.m.		Id.
„	60·0	3 p.m.		
22.	60·0	7 a.m.		Id.
23.	65·0	3 p.m.		
24.	60·0	7 a.m.		Id.
„	67·0	3 p.m.		
25.	62·0	7 a.m.		Id.
„	66·0	3 p.m.		
26.	60·0	7 a.m.		Id.
27.	64·0	3 p.m.		
28.	57·0	7 a.m.		Cloudy.
„	62·0	3 p.m.		
29.	58·0	7 a.m.		Fair.
30.	64·0	3 p.m.		
31.	60·0	7 a.m.		Id.
„	67·0	3 p.m.		

22 days 64°·78 max.
58 ·54 min. } 61°·66 med.

Date.	Thermometer.	Time.	Elevation.	Remarks.
Aug. 1.	60·0	7 a.m.		Thunder. Cloudy.
„	67·0	3 p.m.		
2.	58·0	7 a.m.		Cloudy.
„	66·0	3 p.m.		
3.	60·0	7 a.m.		Id.
4.	67·0	3 p.m.		
5.	60·0	7 a.m.		Id.
„	66·0	3 p.m.		
6	59·0	7 a.m.		Id.
„	68·0	3 p.m.		
7.	61·0	7 a.m.		Id.
„	67·0	3 p.m.		
8.	63·0	7 a.m.		Id.
„	67·0	3 p.m.		
10.	59·0	6 a.m.		Id.
„	64·0	2 p.m.		
11.	59·0	6 a.m.		Id.
„	63·0	2 p.m.		
12.	58·0	6 a.m.		Id.
„	62·5	2 p.m.		

TABLE *continued.*

Date.	Thermometer.	Time.	Elevation.	Remarks.
1825.	°			
Sept. 1.	59·0	6 a.m.		Fair.
2.	65·0	2 p.m.		
3.	60·0	6 a.m.		Id.
"	65·0	2 p.m.		
4.	59·0	6 a.m.		Id.
"	64·5	2 p.m.		
5.	59·5	6 a.m.		Id.
"	65·5	2 p.m.		
6.	57·5	6 a.m.		Id.
"	66·5	2 p.m.		
7.	57·0	6 a.m.		Id.
"	66·0	2 p.m.		
8.	60·5	6 a.m.		Id.
"	63·5	2 p.m.		

19 days 65°·14 max. / 58 ·93 min. } 62°·02 med.

	Thermometer.	Remarks.
1825. October 31 days.	62°·23 max. / 56 ·00 min. } 59°·11 med.	From the 11th rain; 6th shock of an earthquake 12° 25′; comet visible from the 2nd.
1826. February 17 days.	63°·3 max. / 58 ·0 min. } 60°·66 med.	Showery and fair alternately.
March 31 days.	62°·73 max. / 57 ·71 min. } 60°·22 med.	Showery all the month.
April 30 days.	62°·36 max. / 57 ·4 min. } 59°·88 med.	Showery and storms; 5th, lightning struck the hospital of San Juan de Dios.
May 31 days.	62°·7 max. / 57 ·16 min. } 59°·63 med.	Showers and fair alternately.
June 25 days.	58°·28 max. / 56 ·8 min. } 57°·50 med.	Id.
August 25 days.	64°·75 max. / 57 ·00 min. } 60°·87 med.	Fair with some showers.
Septemb. 30 days.	63°·88 max. / 59 ·14 min. } 61°·51 med.	Storms in the afternoon, descend from Cotopaxi, lowering the thermometer from 2° to 6°.

TABLE *continued.*

	Thermometer.	Remarks.
1826. October 31 days.	62°·56 max. 57 ·91 min. } 60°·23 med.	Generally rain with storms of thunder.
Novemb. 30 days.	63°·18 max. 57 ·06 min. } 60°·12 med.	Idem.
Decemb. 31 days.	60°·42 max. 56 ·17 min. } 58°·56 med.	Rainy and foggy.

14 months from July 1825 to December 1826. Mean 59°·97.

Date.	Thermometer.	Time.	Leslie's Hygrom.	Time.	Remarks.
1827.	°		°		
Jan. 1.	54·0	6 a.m.	22·0	1 p.m.	Showery.
„	60·0	2 p.m.			
2.	62·0	2 p.m.	33·0	12	Cloudy.
3.	56·0	6 a.m.	44·0	12	Fair.
„	64·0	2 p.m.			Id.
4.	65·0	2 p.m.	55·0	12	Id.
5.	64·0	2 p.m.	44·0	12	Fair. Rain at night.
6.	57·0	6 a.m.	55·0	12	Fair.
„	65·0	2 p.m.			
7.	64·0	2 p.m.	44·0	12	Fair. Rain at night.
8.	55·0	6 a.m.	46·7	12	Cloudy.
„	62·5	2 p.m.	38·5	2 p.m.	Rain.
9.	59·0	6 a.m.	59·0	12	Fair. 85° in the sun.
„	63·0	2 p.m.			
10.	65·0	2 p.m.	71·5	1½ p.m.	Fair.
11.	64·0	2 p.m.	49·5	12	Cloudy.
12.	58·0	6 a.m.	55·0	12	Id.
„	65·0	2 p.m.			
13.	58·0	6 a.m.	60·5	1½ p.m.	Fair.
„	67·0	2 p.m.			
14.	66·0	6 a.m.	60·5	12	Id.
„	57·0	2 p.m.	38·5	5 p.m.	Cloudy; drops of rain.
15.	57·0	6 a.m.	48·5	12	Id.
„	65·0	2 p.m.	38·6	6 p.m.	Shower.
16.	58·0	6 a.m.	60·5	12	Fair.
„	66·0	2 p.m.	27·5	5 p.m.	Rain.
17.	55·0	6 a.m.	38·0	12	Cloudy: rain at night.
„	65·0	2 p.m.			
18.	58·0	2½	22·0	12	Hail and thunder.
19.	61·0	2 p.m.	33·0	12	Cloudy.
20.	58·0	2 p.m.	16·0	1 p.m.	Rain.
21, 22.	62·0	2 p.m.	33·0	1 p.m.	Cloudy and showers.
23.	64·0	2 p.m.	33·0	1 p.m.	Id.
24.	56·0	6 a.m.			

Table *continued.*

Date.	Thermometer.	Time.	Leslie's Hygrom.	Time.	Remarks.
1827. Jan. 24.	62·0	2 p.m.	33°0	1 p.m.	Cloudy and Showers.
25.	57·0	6 a.m.	22·0	1 p.m.	Id.
,,	61·0	2 p.m.			
26.	60·0	2 p.m.	27·5	1 p.m.	Id.
27.	55·0	6 a.m.	22·0	4 p.m.	Id.
,,	60·0	2 p.m.			
28.	61·0	2 p.m.	22·0	12	Id.
30.	53·0	6 a.m.			
31.	63·0	2 p.m.	49·5	10 a.m.	Fair.

30 days 62°·87 max. / 56 ·13 min. } 59°·5 med. Hygrometer mean 40°·38. Var. 55°.

February 28 days. 65°·86 max. / 57 ·38 min. } 61°·62 med. Hygrometer 57°·07. Var. 55°.

Weather generally fine. The beginning of the month remarkably clear; all the snowy mountains of the horizon of Quito being visible.

March 31 days. 62°·42 max. / 56 ·63 min. } 59°·37 med. Hygrometer 35°·79. Var. 44°.

Weather generally rainy and cloudy.

April 24 days. 64°·0 max. / 54 ·71 min. } 59°·36 med. Hygrometer 33°·55. Var. 55°.

Weather as in the preceding month.

Mean of four months from January to April } 59°·96. Hygrometer 41°·69.

July to December 1826, 14 months .... } 59°·97.

18 months; mean temperature of Quito 59°·96.
July 1825 to April 1827. Hygrometer 4 months 41°·69.

[To be continued.]

LVII. *Letter to the Editor of the Scientific Memoirs respecting a Paper on the Polarization of Heat, in the Sixth Number of that Journal. By* James D. Forbes, *Esq., F.R.S.L. & E. Professor of Natural Philosophy in the University of Edinburgh.*

To Richard Taylor, Esq.

My dear Sir, Edinburgh, Oct. 15, 1839.

THE translation of Mr. Melloni's paper on the polarization of heat, part ii. which appeared lately in the sixth Number of the Scientific Memoirs, contains critical remarks on my experiments in almost every page. I have long hesi-

tated whether to analyse and expose the partial statements which I am sorry to say that paper contains; but reflection convinces me that I need not on this occasion deviate from my general rule of avoiding scientific controversy. This letter, which I request you will have the goodness to insert in the Philosophical Magazine, and also in the succeeding Number of the Scientific Memoirs, is intended simply to request those persons who may feel an interest in this branch of science not to take Mr. Melloni's statement of the evidence upon which my conclusions were founded, but to consult the papers themselves, viz.

On the Refraction and Polarization of Heat. Edinburgh Trans., vol. xiii. and London and Edin. Phil. Mag., vol. vi. 1835.

Researches on Heat. Second Series. Edin. Trans. xiii. Lond. and Edin. Phil. Mag., vol. xiii. 1838.

Researches on Heat. Third Series. Edin. Trans. xiv. Lond. and Edin. Phil. Mag., vol. xiii. 1838.

What has principally determined me to enter into no controversy on the matter is this, that however faulty or unsatisfactory my methods of research may have appeared to the Italian philosopher, he has, with but a *single exception*, confirmed precisely and unequivocally the results which I was the first to announce. That exception is the fact of the variable polarizability of heat from different sources, which is so ingeniously combated in the paper translated in the Scientific Memoirs. On that point I have only to state, that, having examined anew and with every care this curious question, which is considered at length in the third of my papers quoted above, I have not only demonstrated the accuracy of my first assertion, but I have there explained (I believe satisfactorily) the cause of the apparently contrary conclusions obtained by Mr. Melloni. I am, my dear Sir, yours truly,

JAMES D. FORBES.

LVIII. *On the proper Focus for the Daguerreotype. By* JOHN T. TOWSON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE universal interest which the discoveries of Daguerreotype and the photographic art have excited, will, I hope, excuse my soliciting a space in the pages of your scientific Journal, for the purpose of explaining an important fact

which has hitherto escaped observation. It appears from a note appended to page 37 of the English translation of "Daguerre's description," that he does not use an achromatic lens; and from p. 62, that the focus he uses is obtained by advancing or withdrawing the frame of the obscured glass until he obtains the outlines of the subject with the greatest neatness. This method would be most correct if the chemical rays were identical with the luminous rays. If such were the case the effect produced on his plate would be precisely that which had appeared on his obscured glass. But it is a well-known fact, that the chemical rays are more susceptible of refraction than the luminous rays; it is therefore necessary, in order to obtain the neatest effect, that the camera should be adjusted to the focus of the chemical rays.

M. Frauenhofer, by his investigation of the phænomena of the prismatic spectrum, has shown that the index of refraction of each ray is as follows:

	Red.	Orange	Yellow.	Green.	Blue.	Indigo.	Violet.
Flint Glass	1·6277	1·6296	1·6350	1·6420	1·6482	1·6602	1·6710
Crown Glass	1·5258	1·5268	1·5295	1·5330	1·5360	1·5416	1·5465

I also find that the mean index of refraction of the invisible chemical ray is for flint glass 1·693, and for crown glass 1·536. The index for plate glass is also about the mean between those of flint and crown glass.

When we adjust a camera to the point at which the figure appears most distinct we obtain the mean focus of the luminous power of the united ray, because each coloured ray possesses a different degree of illuminating power; therefore the appearance of the figure is mostly influenced by the yellow ray, because it has the greatest degree of illuminating power; and least of all by the violet, because it yields the smallest degree of light.

The proportional light afforded by each ray is as follows: Red ·009, orange ·048, yellow 1·000, green ·440, blue ·084, indigo ·010, and violet ·001. On the other hand, each ray also tends to disturb the distinctness of the figure in proportion to its distance from the mean focus of the pencil to which it belongs; thus two rays would but occasion a similar degree of indistinctness to that which one ray of equal power would if situate at twice the distance from the mean focus of the pencil to which it belongs. The elements of our calculation, in ascertaining the point at which rays of various degrees

of refrangibility produce the most distinct effect, must therefore consist both of the illuminating power of each portion of the spectrum, and its distance from the point required. By a calculation founded on these data, we find that the figure appears most distinct at the focus of the central yellow ray.

It must however be evident that this focus ought not to be used for photographic purposes, since the yellow ray, although it yields the greatest light, produces but a slight degree of chemical action, whilst the chemical effect of the violet ray is greater than that of any other luminous ray, but its illuminating power is the least; the rays that produce even a greater chemical action than all the luminous rays combined possess no illuminating power. It has also been shown by Dr. Herschel that the extreme red ray and the invisible ray beyond the red portion of the spectrum produce a chemical effect of a contrary nature to that of the other rays. These considerations are sufficient to convince us not only that the chemical focus is differently distant from a lens than its luminous focal length, but also to prove that the distance between the two foci is sufficiently great to produce considerable practical results. It therefore becomes an investigation of considerable importance as connected with the photographic art, to ascertain the situation of the mean chemical focus of a lens. In conjunction with the data our previous observations have afforded, the elements of such a calculation must consist of the chemical power of those portions of the spectrum as have not already been noticed, which is as follows. Taking that of the invisible chemical ray as unity, that of the green will be ·01; the blue ·1, the indigo ·3, and the violet ·45. With these data, and adopting the same formula we used in calculating the mean luminous focus, we discover the mean chemical influence to be without the limits of the luminous portion of the spectrum, very near the extreme violet ray, and that for all practical purposes we may find this focus for any lens by multiplying its distance from the point at which the figure appears most distinct by the factor ·969 if it be of flint glass, ·976 of plate glass, or ·984 of crown glass. Thus the chemical focus of a lens whose luminous focus is 16 inches would be if composed of flint glass about 15·504, of plate glass 15·616, or of crown glass 15·744 inches*. To demonstrate

* From this result we might imagine that crown glass would be the best material for photographic lenses. This however is not the case. The least dispersive lenses intercept the greatest number of chemical rays, and therefore those of crown glass, and consequently achromatic lenses, cannot be advantageously employed for photographic purposes. This ob-

the importance of obtaining the chemical focus of a lens, I have inclosed two street views taken on the "improved photographic paper" sold by Mr. Richards of this town. This preparation produces lights which correspond with lights and shades with shades, consequently the effect of a correct focus is more perceptible than would be the case on papers that reverse the tints. The subject of both views is the same; the paper of each is from the same piece; and the times and the lights employed in taking them were as similar as possible, the difference of effect being solely produced by No. 2 having been placed in the mean luminous focus, but No. 1 in the mean chemical focus, discovered by the above formula. On observing the very great difference between the two views, the question immediately occurs, how then does Daguerre produce such clear pictures if he uses the wrong focus? When however we observe the imperfect view, No. 2, we are not to conclude that the luminous focus always produces so little distinctness. During the summer months I have, together with Mr. Hunt of this town, devoted considerable attention to the practice of the photographic art, and have succeeded in obtaining many very tolerably distinct views, although we used the luminous focus of the lens. This we effected by reducing the diameter of the lens or stop to a considerable extent, but by so doing we delayed the process of taking the view. This is also the mode by which Daguerre in a great measure neutralizes the effect of the imperfect focus which it appears he is in the habit of using. By thus reducing the size of the lens of a camera, that aberration of the glass which arises from the use of a wrong focus is diminished in direct proportion to the squares of the diameter of the lens or stop, but the number of rays transmitted is thereby reduced in the same proportion. The diameter of the stop of the camera employed in drawing the inclosed views was equal to one-sixth of the focal length of the lens, whereas it appears from the description of Daguerre's camera that his lens is of less diameter than $\frac{1}{18}$th of its focal length, and the engraving which represents his camera shows a stop of one half that diameter. This being the case the aberration arising from the incorrect focus is reduced to $\frac{1}{36}$th of the amount shown in view, No. 2. But by thus reducing the number of rays transmitted, much of the advantage which would arise from the

servation might be exemplified by reference to several interesting facts, but in so doing we should prematurely anticipate some of the results of an investigation, which my friend, Mr. R. Hunt, is now making relative to the power which various transparent media possess of transmitting chemical rays.

sensitive character of his preparation is lost, and the value of less sensitive modes is reduced in a still greater proportion.

Daguerre informs us, that under very favourable circumstances a drawing may be obtained by exposing his plates in the camera during from three to five minutes. If then, by correcting his focus, he were enabled to use a lens of equal power to the one by which the inclosed drawings were produced, he would be enabled to make the necessary impression in from ten to twelve seconds.

During the discussion which took place at the Institute, after M. Arago had publicly announced the process of Daguerreotype, it was allowed to be a great desideratum that the art might be applied to taking portraits from life. The use of large lenses, which the correction of the focus enables us to adopt, would, I should imagine, render such an application of the art practicable; and the value of each use to which this important invention is applied, must also be increased by a knowledge of the means of obtaining the best possible effect in the least possible time.

I am, Gentlemen, your obedient servant,

JOHN T. TOWSON.

LIX. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions.* By JAMES IVORY, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from p. 109.]

Atmosphere of Air mixed with aqueous Vapour.

CONTINUING to represent the pressure and temperature at the earth's surface by p' and τ', and the like quantities at the height z by p and τ, the symbols (ρ'), (ρ) may be used to denote the respective densities in the case of air mixed with aqueous vapour. When the pressure and density vary, all the gases, and mixtures of gases and vapours, are found to follow the same laws of dilatation and compression; and hence the same equations that express the equilibrium of an atmosphere of dry air, will hold equally in one of moist air. In the present case these equations will therefore be,

$$p = \int \frac{-dz.(\varrho)}{\left(1+\frac{z}{a}\right)^2},$$

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \cdot \frac{(\rho)}{(\rho')}\text{*}:$$

* This equation is equivalent to the one in p. 18 of M. Biot's dissertation, on which that author lays so much stress.

and, if we put

$$\sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{1+\beta\tau}{1+\beta\tau'} = 1 - q, \quad \frac{(\rho)}{(\rho')} = c^{-u},$$

the same equations will be thus written,

$$p = (\rho') \int - d\sigma\, c^{-u},$$
$$p = p'\,(1-q)\,c^{-u}.$$

The three quantities σ, q, u are severally equal to zero at the surface of the earth: so that, by the same procedure as before, we shall obtain these formulas,

$$q = fu - (f - f')\,\frac{u^2}{2} + \&c.$$

$$\sigma = \frac{p'}{(\rho')} \cdot \left\{ u - f \cdot \frac{d\,.\,c^{-u}\,R_2}{c^{-u}\,du} - f' \cdot \frac{dd\,.\,c^{-u}\,R_2}{c^{-u}\,du^2} - \&c. \right.$$

But it is to be observed that, in these expressions, the coefficients f, f', &c., are not exactly the same as in an atmosphere of dry air: for the quantities mentioned, although they have determinate values in the same quiescent atmosphere, depend upon the manner in which the temperature q, or the height z, varies relatively to the density, or to u.

If we suppose that the height z is not very great, so that the powers of q may be neglected, we shall obtain from the foregoing equations,

$$z = \frac{p'}{(\varrho')} \cdot \frac{1+f}{f} \cdot q:$$

and hence

$$\frac{1+f}{f} = \frac{1+\beta\tau'}{\beta} \cdot \frac{(\rho')}{p'} \cdot \frac{z}{\tau'+\tau}.$$

In order to ascertain how far this value is different from the like value in the case of dry air, we must resolve the complex density (ρ') into its elements. The hygrometer will discover the tension of the vapour at the earth's surface; and if ϕ' denote this tension in inches of mercury, and ϱ' be the density of dry air under the pressure p' and at the temperature τ', the following equation is proved in all the late treatises on Natural Philosophy.

$$(\varrho') = \varrho'\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right):$$

by means of which we obtain

$$\frac{1+f}{f} = \frac{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}}{\beta L} \times \frac{z}{\tau' - \tau},$$

$$L = \frac{p'}{\rho'(1+\beta\tau')}.$$

Now the small additional factor in the value of $\frac{1+f}{f}$ is not taken into account in the measurement of heights by the barometer, no distinction being usually made between dry air and moist air. In order to form some estimate of its effect, we may instance the mean atmosphere of our climate, the temperature of which is 50° Fahrenheit; the greatest possible tension of vapour in such an atmosphere is ·36 of an inch of mercury; at a medium, if we make $\phi' = \cdot 18$, and $p' = 30$ inches, we shall have,

$$1 - \frac{3}{8} \cdot \frac{\phi'}{p'} = 1 - \frac{1}{444}.$$

It thus appears that in our climate, when the mean portion of aqueous vapour is mixed with the air, the value of $\frac{1+f}{f}$ is less than it would be if the air were perfectly dry by its $\frac{1}{444}$th part, a quantity too minute to be perceptible in most experiments. A small part only of the refractions depend upon f, about a twelfth part of the whole at the horizon; so that, neglecting the minute variations which f undergoes by the greater or less portions of aqueous vapour mixed with the air, the effect of which on the refractions is insensible, we may assume that it has the same value in all atmospheres. The same thing applies with greater force to the other coefficients f', f'', &c., which having themselves hardly any influence on the refractions, their minute changes in different atmospheres may be wholly disregarded.

If we substitute for (ρ') its equivalent $\rho'\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)$ in the foregoing value of σ, we shall obtain the following equation, which is sufficient for the problem of the refractions in an atmosphere of moist air:

$$\sigma = \frac{1}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}} \cdot \frac{p'}{\rho'} \cdot \left\{ u - f \cdot \frac{d \cdot c^{-u} R_2}{c^{-u}\, du} - f' \cdot \frac{d\, d \cdot c^{-u} R_4}{c^{-u}\, du^2} - \text{\&c.} \right\} \quad \ldots\ldots\ldots\ldots\ldots \text{(10.)}$$

In which expression the coefficients f, f', &c., may be considered the same in all atmospheres, the quantity u varying from zero at the earth's surface to be infinitely great at the top of the atmosphere.

8. In the foregoing analysis, every formula has been strictly deduced from the equations of equilibrium: no quantities have been introduced except such as really exist in nature, and might be determined experimentally, if we had the means of exploring the phænomena of the atmosphere with the requisite accuracy. It may not be improper to notice here an obvious consequence of the equation

$$p = \rho' \int - d\,\sigma\, c^{-u},$$

which holds in an atmosphere of dry air; namely, that the integral

$$\int - d\,\sigma\, c^{-u},$$

being extended from the surface of the earth to the top of the atmosphere, is the analytical expression of $\frac{p'}{\rho'}$, or of the height of the homogeneous atmosphere, that is, of a column of air equiponderant to the whole atmosphere, and every part of which has the same density and the same weight which it would have at the surface of the earth. This height varies only with the temperature, and is thus determined:

$$\frac{p'}{\rho'} = \frac{p'}{\rho'(1+\beta\tau')} \cdot (1+\beta\tau') = \frac{p'}{\mathrm{D}}(1+\beta\tau') = \mathrm{L}\,(1+\beta\tau').$$

In like manner, in an atmosphere of air mixed with aqueous vapour, the same integral is equal to $\frac{p'}{(\rho')}$: and we have

$$\frac{p'}{(\rho')} = \frac{p'}{\varrho'} \cdot \frac{1}{1-\frac{3}{8}\cdot\frac{\phi'}{p'}} = \frac{\mathrm{L}\,(1+\beta\tau')}{1-\frac{3}{8}\cdot\frac{\phi'}{p'}}.$$

Thus the analytical theory agrees in every respect with the real properties of the atmosphere, as far as these have been ascertained; and we now proceed to show that the same theory represents the astronomical refractions with a fidelity that can be deemed imperfect only in so far as the constants f, f', &c., which can only be determined by experiment, are liable to the charge of inaccuracy.

9. The apparent zenith-distance of a star being represented by θ, and the refraction by $\delta\,\theta$, the following formulas have already been obtained (§ 2. equations (2.) and (3.)).

$$d\,.\,\delta\,\theta = \frac{d\,y}{\sqrt{r^2-y^2}},$$

$$y = a\sin\theta \times \sqrt{\frac{1+2\,\phi\,(\varrho')}{1+2\,\phi\,(\rho)}},$$

the quantity $\delta\,\theta$ being supposed to increase from the surface

of the earth to the top of the atmosphere. For the sake of perspicuity, we shall, in the first place, confine our attention to an atmosphere of dry air, in which case it is known by experiment that the refractive power $\phi(\varrho)$ is proportional to the density ρ; so that

$$\phi(\varrho) = \mathrm{K} \times \rho,$$

K being a constant. Adverting to the mode of expression before used, we have

$$\rho = \rho' c^{-u};$$

and hence

$$\phi(\varrho) = \mathrm{K} \times \varrho = \mathrm{K}\,\varrho' . c^{-u},$$

$$y = a \sin\theta \times \sqrt{\frac{1+2\mathrm{K}\rho'}{1+2\mathrm{K}\varrho' c^{-u}}}:$$

and by introducing new symbols in order to abridge expressions,

$$\alpha = \frac{\mathrm{K}\varrho'}{1+2\mathrm{K}\varrho'},$$

$$\omega = 1 - c^{-u},$$

$$y = \frac{a \sin\theta}{\sqrt{1-2\alpha\omega}}.$$

Let this value of y be substituted in the differential of the refraction; then

$$r^2 = (a+z)^2 = a^2\left(1+\frac{z}{a}\right)^2 = \frac{a^2}{\left(1-\frac{\sigma}{a}\right)^2},$$

$$d.\delta\theta = \sin\theta \times \frac{\alpha}{1-2\alpha\omega} \times \frac{d\omega}{\sqrt{\frac{1-2\alpha\omega}{\left(1-\frac{\sigma}{a}\right)^2} - \sin^2\theta}}.$$

In further transforming this expression, it is to be observed that α is a very small fraction less than ·0003; and if the atmosphere extend fifty miles above the earth's surface, $\frac{z}{a}$ or $\frac{\sigma}{a}$ when greatest will not exceed ·012. If we now put

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha\omega,$$

we shall have

$$\frac{1-2\alpha\omega}{\left(1-\frac{\sigma}{a}\right)^2} = \frac{(1-\alpha\omega)^2 - \alpha^2\omega^2}{\left(1-\alpha\omega-\frac{s}{a}\right)^2} = 1+2\frac{s}{a} + 3\frac{s^2}{a^2},$$

the quantities rejected being plainly of no account relatively to those retained. Further, because ω is always less than 1, $\frac{\alpha}{1-2\alpha\omega}$ is contained between α and $\alpha(1+2\alpha)$; and it may be taken equal to α, or to the mean value $\alpha(1+\alpha)$. Thus we have

$$d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)\,du\,c^{-u}}{\sqrt{\cos^2\theta + 2\frac{s}{a} + 3\frac{s^2}{a^2}}}.$$

Again, the formula (9.) gives

$$\sigma = s + a.\alpha\omega = \frac{p'}{\varrho'}.\left\{u - f'.\frac{d.c^{-u}R_2}{c^{-u}du} - f'.\frac{dd.c^{-u}R_4}{c^{-u}.du^2} - \&c.\right\}.$$

Now, $$\frac{p'}{\rho'} = \frac{p'}{\varrho'(1+\beta\tau')}.(1+\beta\tau') = L(1+\beta\tau'):$$
and if we make

$$s.\frac{\varrho'}{p'} = \frac{s}{L(1+\beta\tau')} = x,$$

$$\frac{p'}{\rho'}.\frac{1}{a} = \frac{L(1+\beta\tau')}{a} = i,$$

$$\frac{a.\rho'.\alpha}{p'} = \frac{\alpha}{i} = \lambda,$$

we shall have

$$\frac{s}{a} = i\,x$$

$$x = u - \lambda(1-c^{-u}) - f.\frac{d.c^{-u}R_2}{c^{-u}du} - f'\frac{dd.c^{-u}R_4}{c^{-u}du^2} - \&c.$$

Let $\Psi(u)$ stand for all the terms in this value of x except the first, so that

$$x = u - \Psi(u):$$

from this we deduce by Lagrange's theorem,

$$c^{-u} = c^{-x} - c^{-x}(\Psi)(x) - \frac{1}{2}.\frac{d.c^{-x}\Psi^2(x)}{dx} - \&c.$$

consequently,

$$du\,c^{-u} = dx\,c^{-x} + \frac{d.c^{-x}\Psi(x)}{dx}dx + \frac{1}{2}.\frac{dd.c^{-x}\Psi^2(x)}{dx^2}dx + \&c.$$

By means of the values that have been found, the differential of the refraction can be expressed in terms of one variable x. In making the substitutions, the smallest term of the radical quantity is to be neglected in all the terms of $du\,c^{-u}$, except

the first and greatest; and the denominator of that term is to be expanded. Thus we obtain

$$d\,.\,\delta\theta = \sin\theta\,.\,\alpha(1+\alpha)\,.\left\{\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\right.$$

$$.\left(c^{-x}+\frac{d\,.\,c^{-x}\,\Psi(x)}{d\,x}\right)$$

$$+\frac{1}{2}\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\,.\,\frac{d\,d\,.\,c^{-x}\,\Psi^2(x)}{d\,x}$$

$$\left.-\frac{3}{2}\int\frac{d\,x\,.\,c^{-x}\,.\,i^2\,x^2}{(\cos^2\theta+2\,i\,x)^{\frac{3}{2}}}\right..$$

In order to estimate the relative magnitude of the several parts of this formula we must find the numerical values of the quantities α and i. If η stands for the refraction at 45° of altitude, determined very exactly from many astronomical observations, we shall have

$$\alpha = \eta\,(1-2\,i+2\,\eta),$$

as will readily appear from the formula according to Cassini's method given in § 1. MM. Biot and Arago have ascertained the value of α with great exactness in a different way, by means of experiments on the gases with the prism. In some of the best attempts to determine α, the refractions at 45° of altitude, being reduced to the barometer 29·6 and to the temperature 50° Fahr., are as follows:

	″
Dr. Brinkley	57·42
De Lambre	57·58
Bessel, Tab. Reg.	57·55
Experiments of MM. Biot and Arago	57·65
Mean	57·55

It appears that Bessel's determination has the best claim to be preferred: but as it differs very little from De Lambre's result, which is adopted in the paper of 1823, the same value will be retained in the calculations which follow. According to De Lambre, the value of α is 60″·616* at the temperature 0° centigrade, and the barometric pressure 0^{m}·76: wherefore, when the temperature is 50° Fahrenheit, and the pressure 30 inches (= 0^{m}·762), we shall have

$$\alpha = 60{\cdot}616\times\frac{762}{760\times1{\cdot}0018}\times\frac{1}{1+\frac{18}{480}} = 58''{\cdot}47:$$

* *Tableaux Chronomiques, publiées par le Bureau des Longitudes de France.*

and in parts of the radius,

$$\alpha = \cdot 0002835.$$

It has been found that L = 4347·8 fathoms at 0° centigrade or 32° Fahr.: wherefore, if we make a = mean radius of the earth = 3481280 fathoms, we shall have at the temperature of our climate, or 50° Fahrenheit,

$$i = \frac{\mathrm{L}\,(1+\beta\tau')}{a} = \frac{4347{\cdot}8\left(1+\frac{18}{483}\right)}{3481280} = \cdot 0012958;$$

and hence

$$\lambda = \frac{\alpha}{i} = \cdot 21878.$$

We can now inquire into the values of the last two terms of the foregoing formula for the refraction, both of which are very small. With respect to the first of them, we have

$$\Psi(x) = \lambda\,(1-c^{-x}) + f.\frac{d\,.\,c^{-x}\,\mathrm{R}_2}{c^{-x}\,d\,x} + f'.\frac{d\,d\,.\,c^{-x}\,\mathrm{R}_4}{c^{-x}\,d\,x^2} - \&c.:$$

and, by performing the differential operations,

$$\Psi(x) = \lambda\,(1-c^{-x}) + f(\mathrm{R}_1+\mathrm{R}_2) + f'\,(\mathrm{R}_2+3\,\mathrm{R}_3+\mathrm{R}_4);$$

and, by substituting the values of the functions,

$$h = 2f - \lambda = \cdot 22566$$

$$\Psi(x) = -h\,(1-c^{-x}) + f x + 4f'\left(1 - x + \frac{3\,x^2}{8} - \frac{x^3}{24} - c^{-x}\right).$$

It might not be very objectionable to neglect the term multiplied by f', for the same reasons that the terms which follow it are neglected, that is, both on account of the nature of the functions and because the coefficients are small: but, in order to leave no room for scruples respecting accuracy, the square of the entire expression set down, may be thus represented:

$$\Psi^2(x) = \mathrm{G} - 8\,h f'\,.\,\mathrm{G}' + 8 f f'\,.\,\mathrm{G}'' + 16 f'^2\,.\,\mathrm{G}'''.$$

The integral in the term under consideration is greatest when the radical quantity in the denominator is least, that is, when $\cos\theta = 0$: and if the integration be performed between the limits $x = 0$, $x = \infty$, we shall obtain a result greater than if the integral were extended only to the top of the atmosphere. Now we have,

$$\mathrm{G} = h^2\,(1 - 2\,c^{-x} + c^{-2x}) + 2\,h f\,.\,x\,c^{-x} - 2\,h f\,.\,x + f^2\,.\,x^2:$$

and, by operating on the terms separately, the part of the integral depending on G, will be as follows:

$$\int_0^{\infty} \frac{d\,x}{\sqrt{2\,i\,x}}\,.\,\frac{d\,d\,.\,c^{-x}\,\mathrm{G}}{d\,x^2} =$$

$$\frac{\sqrt{\pi}}{\sqrt{2i}} \times \left(h^2(1-4\sqrt{2}+3\sqrt{3})-3hf(\sqrt{2}-1)+\frac{3}{4}f^2\right)$$
$$= \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00216.$$

The other parts depending on G′, G″, G‴ are complicated; but they are troublesome more on account of the number of terms they contain than from any difficulty in the integrations. The following results have been obtained:

$$8hf' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd.c^{-x}\,G'}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 01759,$$

$$8ff' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd.c^{-x}\,G''}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 02043,$$

$$16f'^2 \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd.c^{-x}\,G'''}{dx^2} = +f'^2 \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00855.$$

Collecting all the parts, the term sought is found, viz.

$$\frac{\alpha(+\alpha)}{2} \cdot \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd.c^{-x}\,\Psi^2(x)}{dx^2} =$$

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times (\cdot 00108 - f' \times \cdot 00142 + f'^2 \times \cdot 00427).$$

To this must be added the other term, which, being integrated in the same circumstances, gives,

$$-\frac{3}{2} \cdot \int_0^\infty \frac{dx\,c^{-x}}{\sqrt{2ix}} \times \frac{ix}{2} = -\frac{3}{8} \cdot \frac{\sqrt{\pi}}{\sqrt{2i}} = -\frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00049.$$

It thus appears that the two small terms of the expression of the refraction are, together, equal to

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \cdot (\cdot 00059 - f' \times \cdot 00142 + f'^2 \times \cdot 00427):$$

and as
$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} = 2036''\cdot 5,$$

the greatest amount of both is about 1″.

The whole refraction will therefore be thus expressed:

$$d.\delta\theta = \sin\theta \times \alpha(1+\alpha) \cdot \int \frac{dx}{\sqrt{\cos^2\theta + 2ix}} \cdot \left(c^{-x} + \frac{d.c^{-x}\,\Psi(x)}{dx}\right),$$

with the assurance that the error cannot exceed 1″. If we substitute what $\Psi(x)$ stands for, we shall have

$$d.\delta\theta = \sin\theta \times \alpha(1+\alpha) \times \int \frac{dx}{\sqrt{\cos^2\theta + 2ix}} \times$$

$$\left(c^{-x} + \lambda . \frac{d.(-x - c^{-2x})}{dx} + f . \frac{dd . c^{-x} R_2}{dx^2} + f' \frac{d^3 . c^{-x} R_4}{dx^3}\right) + \&c.$$

This expression being regular, it may be continued to any number of terms, and it has the advantage of being linear with respect to the coefficients. Adverting to what x stands for, it will appear that $L \times x$ is nearly equal to s, or to z, that is, to the elevation in the atmosphere; so that, if we suppose the greatest height of the atmosphere is $10 \times L$, or about fifty miles, the greatest value of x will be 10; and all the integrals in the foregoing expression must be taken between the limits zero and 10. But the quantity c^{-x} is so small when x has increased to 8 or 10, that the results are not sensibly different whether the integrals be extended to those limits or be continued to infinity. By substituting the values of the functions, the expression of $\delta\theta$ will take this form:

$$\delta\theta = \sin\theta \times \alpha(1+\alpha) \times \Bigg\{ \int \frac{dx\, c^{-x}}{\sqrt{\cos^2\theta + 2ix}}$$

$$+ \lambda \int \frac{dx\,(2c^{-2x} - c^{-x})}{\sqrt{\cos^2\theta + 2ix}}$$

$$- f \int \frac{dx}{\sqrt{\cos^2\theta + 2ix}} . \left(4c^{-2x} - 3c^{-x} + x c^{-x}\right)$$

$$+ f' \int \frac{dx}{\sqrt{\cos^2\theta + 2ix}} . \Big(8c^{-2x} - 8c^{-x} + 7xc^{-x}$$

$$-2x^2 c^{-x} + \frac{x^3 c^{-x}}{6}\Big)$$

$$- f'' \int \frac{dx}{\sqrt{\cos^2\theta + 2ix}} . \Big(16c^{-x} - 16c^{2-x} + 16xc^{-x}$$

$$- \frac{15}{2} x^2 c^{-x}$$

$$+ \frac{11}{6} x c^{-x} - \frac{5}{24} x^4 c^{-x} + \frac{x^5 c^{-x}}{120}\Big).$$

In order to illustrate the rapidity with which the terms decrease, it may be proper to find the limit of $\delta\theta$, by making $\cos^2\theta = 0$, and integrating between the limits $x = 0$, $x = \infty$; which limit is not sensibly different from the refraction at the horizon. Now it will be found that, in the circumstances mentioned,

$$\delta\theta = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \Big\{ 1 + \lambda\left(\sqrt{2} - 1\right)$$

$$-f\left(2\sqrt{2}-\frac{5}{2}\right)$$
$$+f'\left(4\sqrt{2}-\frac{91}{16}\right)$$
$$-f''\left(8\sqrt{2}-\frac{2895}{256}\right)$$
$$-\ \&c.:$$

or, in seconds,

$$\delta\theta = 2072''{\cdot}46 - f'\times 62{\cdot}4 - f''\times 10''{\cdot}2 - \&c.$$

From this calculation it appears that the term multiplied by f'' and all the subsequent terms are too small to be sensible; and as f' is much less than f, even the term multiplied by f' can hardly exceed a few seconds at low altitudes. There is great probability that the horizontal refraction is very near 34′ 30″, and does not exceed this quantity.

To prepare the foregoing expression of $\delta\theta$ for integration, put

$$m = 10,\ \frac{\sqrt{2\,i\,m}}{\cos\theta} = \tan\phi,\ e = \tan\frac{\phi}{2};$$

then

$$\cos^2\theta = \frac{(1-e^2)^2}{4\,e^2}\times 2\,i\,m,$$

$$\sqrt{\cos^2\theta + 2\,i\,m} = \frac{\sqrt{5\,i}}{e}.\sqrt{(1-e^2)^2+4\,e^2.\frac{x}{m}} = \frac{\sqrt{5\,i}}{e}.\Delta:$$

and we shall have

$$\left.\begin{aligned}\delta\theta = \sin\theta\times\frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}}\times\Big\{&\int_0^m\frac{e\,d\,x}{\Delta}.\,c^{-x}\\ &+\lambda\int_0^m\frac{e\,d\,x}{\Delta}.\,(2\,c^{-2x}-c^{-x})\\ &-f\int_0^m\frac{e\,d\,x}{\Delta}.\,(4\,c^{-2x}-3\,c^{-x}+x\,c^{-x})\\ &+f'\int_0^m\frac{e\,d\,x}{\Delta}.\,(8\,c^{-2x}-8\,c^{-x}+7\,x\,c^{-x}-2\,x^2\,c^{-x}\\ &\qquad+\frac{x^3}{6}\,c^{-x}).\end{aligned}\right\}\ (C.)$$

For the sake of abridging, the several integrals in succession may be represented by Q_0, Q_1, Q_2, Q_3; so that the value of $\delta\theta$ will be thus written:

$$\delta\theta \sin\theta\times\frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}}.\,(Q_0+\lambda,\,Q_1-f,\,Q_2+f',\,Q_3).$$

[To be continued.]

LX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xiv. p. 520.]

April 10.—A PAPER was read, "On as much of the Transition or Grauwacke system as is exposed in the counties of Somerset, Devon, and Cornwall," by the Rev. David Williams, F.G.S.

The author commences by stating, that his views of the general structure and arrangement of the country are original and independent, but that he does not in the least impugn the originality of the observations and inferences of other geologists. He mentions, that in a communication read before the British Association at Dublin (1835), he used the following expression in remarking on the broad outline of the structure of Devonshire with respect to the relative position of the strata containing plants and culm: "the clay slate (without the intervention of gneiss or mica-slate) dips away from the granite of Lundy on the one hand, and from the granite of Dartmoor towards it on the other;" and that in a paper sent to the Meeting of the British Association at Bristol (1836), but received too late to be read, was inserted this passage: "the same beds being brought up to the surface at either extremity" (Exmoor and the north of Cornwall) "contain in their great intermediate trough all the strangely contorted rocks and carbonaceous shales we there witness."

Mr. Williams then alludes to an error which he made in the paper read at Dublin, by considering the mineral axis of Dartmoor to be composed of the strata he calls the "Morte Slates," but which he corrected in a paper read at Liverpool (1837); he notices also another error which he had made in supposing that the same beds (the Morte slates) were brought up among the granite of Dartmoor, and which he did not discover till the spring and summer of 1838, when he perceived that "the two superior members of the North Devon group, Nos. 7. and 8. are brought up in the south in precisely the same order and relation in which they descend on the north," having previously overlooked this natural simplicity of arrangement.

The chief objects of the paper are to show, that the strata can be divided into certain groups, distinguished by well-marked lithological characters; and that there is a gradual passage from the lowest part of the uppermost or culm deposit into the series next below it, and that similar passages are presented in each of the other underlying groups. To the intermediate strata the term neutral is applied.

The whole of the beds are assigned to the transition or graywacke class, and are arranged in descending order under the following nine heads, the topographical names being derived from the localities where the strata are best exposed:—9. Floriferous slates; 8. Coddon Hill grits; 7. Trilobite slates; 6. Wollacomb sandstones; 5. Morte slates; 4. Trentishoe slates; 3. Calcareous slates

of Linton; 2. Foreland and Dunkerry sandstone; 1. Cannington Park limestone. Only 9. 8. and 7. are described in the paper; the other six, confined, the author believes, to the north of Devonshire and the south of Somersetshire, being reserved for future consideration.

9. *Floriferous slates and sandstones.*—This term is proposed for the series of beds containing culm, to avoid the ambiguity of the word "carbonaceous," and as preferable, in the author's opinion, to "culmiferous," plants being very generally distributed, and culm confined to a small area. The sandstones are finely micaceous, tough, externally of a rusty or dull purple colour, and internally of a dull olive, and they are stated to be totally distinct from any others in the country. The shales or slates are commonly dark-coloured and friable, but at Forrabury and Bos Castle they constitute roofing slates, resembling those of the inferior groups, though much deteriorated by a combination of pyritous anthracite. One variety, called Adder Limestone, is a fine hone slate. The culm forms great insulated elliptical "bunches," sometimes gradually thinning out, and sometimes being suddenly nipped off. The strata are strangely contorted, and these disturbances have entailed on the country its physical features of rapidly succeeding hills and valleys; but Mr. Williams conceives, that the curvatures are confined to No. 9. and the two upper divisions of No. 8. and that they are due to lateral pressure produced by the upheaval of the granite of Dartmoor. The area occupied by the "floriferous deposit" is stated to be 50 miles in a west and east direction, and 25 in a north and south.

8. *Coddon Hill Grits.*—On the confines of this formation the floriferous sandstones become thin-bedded and coarsely laminated, and after a series of alternations and gradual transitions, are finally succeeded by the well-characterized Coddon Hill grits. This series is divided by the author into grits, limestones, and dark slates, connecting the floriferous sandstones (9.) with the trilobite slates (7.); and Mr. Williams asserts, that more regular passages from one system of beds to another cannot exist, there being no want of conformity, and that as the constituents of one deposit gradually decrease those of the other gradually increase. The grits are stated to be lithologically distinct from any other in the country. They are slightly calcareous, fine-grained, flinty, thin-bedded, and dark-coloured, but often striped of different tints; and from containing a varying proportion of felspar, occasionally assume, on decomposition, a resemblance to some of the harder chalks. The wavelite of Devonshire occurs in these grits. The following localities are mentioned where the passage from the floriferous strata into the Coddon grits, and thence into the trilobite slates, may be advantageously examined: the neighbourhood of Bampton, Morebath, where the turnpike road to Hatchet intersects the grits—the back of Swimbridge, four miles east of Barnstaple—Rumson Lane, a mile south of Barnstaple, and Fremington Pill, below Pen-hill, on the west of Barnstaple. Organic remains are very rare in the grits,

Mr. Williams having found only a few fragments of Crinoidea and a chambered univalve.

The grits are associated, about the middle of the series, with large insulated lenticular masses composed of beds of dark limestones alternating with strata of black shale, containing plants and flakes of anthracite; also Goniatites and Posidonia. These lenticular masses may be traced, in the north of Devon, from Barnstaple to Bampton, and in the south from Launceston to Drewsteignton. To the east of Bampton and Drewsteignton the shales not only thin out, and the whole mass becomes calcareous, but the author says, that there is an upper suite of thick-bedded coral limestones. These changes are stated to take place at Hockworthy, Holcomb Rogus, Westleigh, Chudleigh, and Ashburton, emerging at each locality except the last, from below the floriferous slates, and accompanied by the Coddon Hill grits. At Ashburton, however, he states, that a fault brings the limestone abruptly in contact with the trilobite slates, the passage beds not being exhibited.

The Coddon Hill limestones are succeeded by the lowest division of No. 8, consisting of the series of slaty beds which forms the passage into the trilobite slates (No. 7.).

7. *Trilobite Slates.*—This group is characterized, in some localities, by an abundance of trilobites, particularly in the north of Devon, and at Landlake in the south. It constitutes the low southern flank of Exmoor, ranging from Baggy and Diamond Points on the British Channel eastward to Shawley; and Mr. Williams conceives that it constitutes the south of Devonshire and the whole of Cornwall, with the exception of the granitic and other igneous masses. The limestones of Trenalt, Petherwin, Landlake, Plymouth, Newton Bushell, Denbury, and Torbay, are placed in it by the author; but in the north of Devon he knows only two localities at which limestone has been observed in this division. Organic remains are abundant in the calcareous beds, and are well preserved. The author estimates the thickness of the group to be $8\frac{1}{2}$ miles.

The strata in the north of Devon and south of Somerset inferior to No. 7, Mr. Williams proposes to describe in another paper.

April 24.— A paper was first read "On the Climate of the newer pliocene tertiary period," by James Smith, Esq., F.G.S.

During an examination of the fossils contained in the marine beds which indicate the latest changes in the relative level of sea and land in the west of Scotland, Mr. Smith observed, that many of the most common shells in the raised beds of the basin of the Clyde are identical with species found by Mr. Lyell at Uddevalla in Sweden*; and he has been induced to conclude from the arctic character of the testacea, that the climate of Scotland during the accumulation of these beds was colder than it is at present.

On showing some of the fossils, which are apparently extinct, to Mr. Gray, that naturalist noticed their great resemblance to arctic species. The shells still living, though not known on the coasts of Great Britain, but found in the raised deposits of the Clyde,

* Phil. Trans., 1835, Pl. 1.

M. Deshayes has determined to be inhabitants of the northern seas, viz. *Natica clausa,* which occurs as far north as Spitzbergen; *Fusus Peruvianus,* erroneously considered by Lamarck to exist on the coasts of Peru, but which is an inhabitant of the seas at the North Cape; *Tellina proxima, Astarte multicostata, Turbo expansus, Velutina undata,* (also on the coast of Newfoundland); and *Pecten Islandicus,* erroneously considered by some conchologists, according to M. Deshayes and Mr. G. Sowerby, to occur in a living state on the coast of Scotland.

The *Cyprina Islandica,* which is abundant in the raised deposits, Mr. Smith has not found alive in the waters of the Firth of the Clyde.

The following summary is given in the paper of shells found in the newer pliocene deposits in the British Isles.

Marine	190 species.	
Land and fresh water	57	
		247
Of these there are recent British		
Marine species.................. 166		
Land and fresh water 54		
	220	
	27	
Recent in Arctic seas 7		
European and Indian seas 1		
Extinct or unknown 19		
	27	

Mr. Smith also mentions the occurrence in the newer pliocene of Sicily, of several species now found living only in more northern European seas; and he infers from them, that the climate of Sicily was at one period colder than it is at present. Four species are mentioned in the paper, *Panopœa Bivonæ, Bulla ampulla, Arca papillosa,* and *Bulbus Smithii.*

A paper was then read, entitled, "Remarks on some fossil and recent shells, collected by Capt. Bayfield, R.N., in Canada," by Charles Lyell, Esq,, V.P.G.S.

Several eminent conchologists having observed that the English crag contains shells, which seem to indicate a somewhat colder climate than that which now prevails in our latitude; and it having been supposed that a similar inference may be deduced, with still greater certainty, from the abundant occurrence of many arctic species in the marine newer pliocene strata of Scotland and Ireland, Mr. Lyell was induced to examine carefully a collection of shells procured by Capt. Bayfield, and consisting partly of fossils from the most modern tertiary deposits bordering the Gulf of Saint Lawrence, and partly of recent testacea from the gulf itself.

The shells were obtained principally at Beauport (lat. 47°) 2 miles below Quebec and 100 feet above the St. Lawrence, but similar species are met with on the north side of the St. Charles, 3 miles from Beauport, and at Port Neuf, 40 miles above Quebec, in the latter instance at heights varying from 50 to 200 feet above the level of the river.

The deposits near Quebec fill a valley formed in a horizontal limestone, containing Trilobites and Orthocera, and they resemble those forming in the bed of the St. Lawrence. They consist of strata of sand, gravel, and stiff blue clay, the last composing the bottom of the series, and the first the uppermost part. Numerous boulders occur at different levels, not resting upon each other, but dropped apparently at widely distant intervals of time, from masses of ice on which it is supposed they had been floated. Some of the shells are broken, but many are perfect, and have both their valves together; and it is impossible to imagine that the clay, sand, gravel, and boulders could have been drifted together, into their present position, by a violent rush of water, as the fragile *Terebratula psittacea* is found perfect, and with its interior appendages complete.

On first examining the shells, which are found principally in the upper sandy bed, Mr. Lyell was struck with their great resemblance to those which he had collected at Uddevalla in Sweden. The *Saxicava rugosa*, so predominant there, is particularly mentioned by Capt. Bayfield as the most abundant shell in the tertiary strata of the St. Lawrence; and the *Natica clausa* and *Pecten Islandicus* are very common at each locality. The fossils of Beauport, however, considered as a whole, by no means agree with the marine shells inhabiting the Gulf of St. Lawrence, but, as far as they have been examined, possess a decidedly arctic character, the species ranging from the Gulf to the border of the north polar circle, or being found in the newer pliocene of Scotland and Sweden; and on the contrary many of the most conspicuous of the living testacea of the St. Lawrence are wanting in the tertiary deposits.

The following list of some of the fossil species is given by Mr. Lyell on the authority of Dr. Beck: *Mya truncata* (var.), found fossil in Bute, and living in the St. Lawrence; *Mya arenaria* and *Saxicava rugosa*, recent in the Gulf of St. Lawrence; *Tellina calcarea*, fossil at Bute; *Tellina Grœnlandica*, which exists in the Gulf of St. Lawrence and at Icy Cape; *Mytilus edulis*; *Pecten Islandicus*, found living in the North Sea, and fossil in Scotland; *Terebratula psittacea*, which occurs on the coasts of Greenland and the Feroe Islands; also at places intermediate between them and the entrance of the Baltic; *Natica clausa*, recent in Greenland and fossil at Uddevalla; *Scalaria Grœnlandica*, *S. borealis*, *Tritonium fornicatum*, *T. Anglicanum*, all now existing in the Greenland seas, the last being considered by some authors as a variety of *Buccinum undatum*, and the *T. fornicatum* being also found living on the Irish coast, and fossil at Dalmuir and in Scotland. On the other hand, many of the shells living in the Gulf of St. Lawrence and most conspicuous for their size, are wanting in the collections of fossils hitherto obtained, as the *Mactra solidissima*, *Erycina Labradorica*, *Purpura*, allied to *P. Lapillus*, *Natica Heros*, and *Rostellaria occidentalis*.

The torrents and rivers which flow into the St. Lawrence wash down annually into that estuary great numbers of tertiary fossil shells, so that they become mingled with the living testacea. The latter, however, may be generally distinguished by retaining their colour, animal matter, or ligaments; but it is more difficult to di-

stinguish those shells which have been derived exclusively from the tertiary beds. Nevertheless, Mr. Lyell has little doubt in assigning to them the specimens of *Balanus Uddevallensis* and the Fusus allied to *F. lamellosus*, which have been dredged up off Cape Bic, as they are all in the same condition as the Beauport fossils.

The climate of Canada being now excessive, it is natural to find in the Gulf of St. Lawrence many northern and arctic species, without any mixture of tropical forms, for the latter cannot resist severe cold, though they range far towards the southern polar latitudes, where a low mean annual temperature prevails. Mr. Lyell, therefore, conceives that during the period immediately antecedent to the present, the climate of Canada was even more excessive than it is now; and that the shells resembled still more closely the small assemblage now living in high northern latitudes. He is also of opinion, that this extreme cold may have coincided with the era of the principal transportation of erratic blocks, an inference supported by the masses of rock irregularly dispersed among the clay. He further believes, that a more equable though cold climate may have preceded immediately that condition; and that there may have been more than one oscillation of climate at the modern period, the last having been connected with the geographical changes which upheaved the shelly deposits of Canada 200 feet above the lèvel of the St. Lawrence, and converted them from submarine deposits to dry land.

An extract was next read from a letter addressed to Dr. Fitton by Herr Roemer, of Hildesheim, on the Wealden of the North of Germany.

The Wealden formation, including the Purbeck stone, is very extensively developed in the north of Germany, and is overlaid by a great argillaceous deposit containing marine shells, similar both to the oolitic and cretaceous systems. Of the fossils found in the Wealden of England, almost every species occurs in Germany, including even the minute *Cypris tuberculata*, *C. granulosa*, and *C. Valdensis*. Last autumn, Herr Roemer discovered the Wealden with its characteristic shells, near Bottingen, in the High Alps. He possesses also the *Lepidotus Mantelli* of the English Wealden, from Saxony. The Portland sand occurs in the north of Germany, but the Portland stone and the Kimmeridge clay are so intimately connected by their fossils, that the intermediate sandy beds cannot be considered as a separate deposit. The chalk with flints occurs possibly in the Hartz. The greensand series is extensively developed, the Flammenmergel of Hausmann being the upper greensand of England, and the quader-sandstein the lower. Herr Roemer believes that the gault also exists in Northern Germany.

A paper was then read on the classification of the older rocks of Devonshire and Cornwall, by the Rev. Professor Sedgwick, F.G.S., and Roderick Impey Murchison, Esq., F.G.S.*

In a communication read in 1837, the authors explained their general views respecting the older rocks of Devon and Cornwall, but having recently changed one part of their classification, they

* [See Lond. and Edinb. Phil. Mag. vol. xiv. p. 241, 354, 358; and pres. vol. p. 109, 293. Edit.]

have hastened to place their reasons for doing so upon record, before the Geological Society. On three out of four of the essential points in their former communication, the authors' views remain unchanged; they adhere to the belief, which they were the first to put forth, that the greater portion of Devonshire belongs to the true carboniferous system, and that the succession and lithological characters of the different mineral masses in North and South Devon, which they then pointed out, remain unaltered. In proof of this there were suspended, during the reading of the paper, the same sections as were exhibited at Bristol in 1836. The change, therefore, which they propose, is to remove the lowest rocks from the Cambrian and Silurian systems to the old red; and their reason for making this alteration is founded on zoological evidence recently obtained, which shows that the organic remains of these deposits are of a peculiar character, approaching in the upper division, the fossils of the carboniferous strata, and in the lower, those of the Silurian system; as well as upon the previously ascertained regular sequence or passage from the carboniferous strata, through all the subjacent series of deposits.

The fossil plants of the culm basin having been formerly determined to be, as far as recognizable, true coal measures remains, and the deposit having been therefore assigned to the era of the carboniferous system, the order of superposition being also clear, the strata underlying the coal basin might naturally be referred to the old red sandstone, if the organic remains found in them, belonged to a natural group, intermediate between the fossils of the carboniferous and Silurian systems. Subsequent examination has proved that such is the case; but this distinction could not have been ascertained had not Mr. Murchison published his work on the Silurian system.

In the order of sequence there is now no difference of opinion between the authors and Mr. De la Beche and Mr. Williams, the only point on which the agreement is not common, being the class to which the formations should be assigned.

The authors then explained that their sections both in S. Devon and N. Cornwall indicate, with some limited exceptions, a passage downwards, the transition being stratigraphically true, whether the beds be examined along the banks of the Taw, near Barnstaple, on the north, or to the west of Launceston, on the south of the great trough.

The authors next gave an approximate list of the fossils, collected by themselves or placed at their disposal by the Rev. R. Hennah, Major Harding, and the Rev. D. Williams, referring them to the great mineral groups to which they belong, both in North and South Devon.

Descending order in North Devon.—The shells in the uppermost group, beneath the culm, as at Barnstaple, in the North of Devon, and South Petherwin, near Launceston in the south, approach generally forms of the carboniferous system, consisting of Goniatites of new species, and of spined Producti and Spirifers, entirely unlike

the species found in the Silurian system, but resembling those obtained in the mountain limestone. The same group contains also new species of Trilobites and Crinoidea.

In the next underlying formation in the north, or the sandstone group, ranging from Baggy Point by Marwood and Sloly, occur new species of *Cucullæa, Avicula? Cypricardia*, and *Orthocera*; one cast also has been obtained, undistinguishable from *Bellerophon globatus* of the Silurian system. In the same series are found casts of plants of considerable size, but in Professor Henslow's opinion, quite distinct from any known coal measures remains.

In the third descending group, but few fossils have yet been found, yet it has been ascertained to contain one of the varieties of Producta common in the overlying groups, and similar to the spinous species of the mountain limestone; also a coral (*Favosites polymorpha,*) previously found in England only in the Upper Silurian rocks.

The next descending series of beds, or the arenaceous deposits of Linton, contains few fossils, except towards its lower part, where calcareous matter re-appears, and in that portion a Spirifer has been obtained resembling the *S. attenuatus* of the mountain limestone, and a new species of Orthis, a genus characteristic of the Silurian system.

In the Quantocks, which the authors consider as formed of the oldest strata in North Devon, organic remains appear to be rare, the principal hitherto procured consisting of *Favosites polymorpha.*

South Devon.—Having thus shown that in North Devon there is a regular succession of strata characterized by distinct fossils differing more and more in descending order from the organic remains of the mountain limestone, and approaching those of the Silurian system; the authors proceed to enumerate the order of the groups and the imbedded fossils in South Devon and the North of Cornwall. They show a similarity of succession of deposits and of organic remains in the upper groups, but they state that in consequence of the protrusion of the granite, there is in the lower a considerable difference in mineral type, especially south of Dartmoor. They refer, however, to their former memoir for ample details respecting these counties, and for proofs that they were correct in placing the great calcareous masses of Plymouth and Chudleigh on the same parallel as the lowest calcareous strata of North Devon.

In conclusion, the authors show, that the variation in Devonshire and Cornwall from the ordinary type of the old red sandstone in Herefordshire and adjoining counties, cannot be admitted as a valid argument against assigning the slates and sandstones of these counties to that system, because the variations in composition of other formations within limited areas is equally great. They show also that the absence of the true carboniferous limestone in Devonshire cannot disprove their present classification, because in Western Pembrokeshire that limestone is wanting, and the coal measures rest on older formations.

In consequence of mineral character being no longer indicative

of age, and the term greywacke being lithologically applicable to beds of every class of rocks, and as Devonshire affords the best type of the fossils of this intermediate system, the authors propose to substitute the term *Devonian* for old red sandstone; and they hope that the organic remains, discovered in that county, will enable continental geologists to detect in their own country, a system of strata hitherto supposed to be almost peculiar to the British Isles.

The authors acknowledge the assistance they have received from Mr. J. Sowerby; and that Mr. Lonsdale first suggested, from their fossil contents, that the limestones of S. Devonshire might prove to be the representatives of the old red sandstone.

A paper was afterwards read on the structure of South Devon, by Robert A. C. Austen, Esq., F.G.S.

This communication is supplementary to a memoir read in 1837*, and its object is to show the general relations of the various bands of slates, limestones, and sandstones in South Devon.

Commencing with the older deposits east of the Teign, there appear—

1st. Slates, but of which little is seen.

2nd. A band of black stratified limestone of variable thickness and slaty structure. It contains much carbonaceous matter, thin seams of anthracite, also corals and Brachiopoda. It is associated with irregular beds of contemporaneous trap. The band is stated to range from Staple Hill on the east, through Bickington, Ashburton, Buckfastleigh, and Dean, near which the limestone ends; but the calcareous slate and limestone of the south of Cornwall, Mr. Austen considers to be of the same age. These beds dip south.

3rd. Fine-grained schists and roofing slates.

4th. The Plymouth limestones, which cannot be traced westward further than Whitesand Bay, but to the eastward they are considered by Mr. Austen to be represented by the limestones of Dunwell, Shilstone, Ugborough, Fowley-cumber, North Huish, Stoverton, Great and Little Hampston, &c.

5th. An arenaceous deposit, often coarse and resembling old red sandstone; but sometimes conglomeratic, and then not distinguishable from the new red of Devonshire. Its upper conglomeratic portion ranges from Plymouth Sound and Bigbury Bay, to Modbury and Blackdown; its lower portion cuts the Dart a little below Totness, and rises into lofty hills, east of a line passing through Berry Pomeroy, Marldon, Cockington, and Barton. It contains limestone south of Yealmpton, and at Sequers Bridge; also several thin bands on the Dart, and beds at Berry, Marldon, Collaton, and Yalberton. Organic remains are not uncommon in this arenaceous division. Only the fine-grained beds show a slaty cleavage. The limestone is confined to its northern limit, and has a southwardly dip; but all the lines of roofing slate are to the southern with either vertical or northern cleavage dips. As the intermediate country about

* Proceedings, vol. ii., p. 584. [or L. & E. Phil. Mag., vol. xiii. p. 564. —Edit.]

Modbury presents many undulations, Mr. Austen suggests that the slate beds of the south may be the equivalents of the limestone on the north; in which case the passage downwards into the mica slate and gneiss of the Prawle Point may be the equivalents of No. 4, in a metamorphic condition.

6th. The limestones of Torbay, &c., which are said to constitute the newest deposits of the series, not being covered by any formation into which they pass.

The carbonaceous rocks of central Devon are stated by Mr. Austen to form no part of the above system, but to rest upon it unconformably.

May 8. An extract from a letter addressed to Mr. Murchison by Mr. Miller of Cromartie, was read.

The fish beds in the old red sandstone of the neighbourhood of Cromartie, are very extensive. They are overlaid, where not denudated, by a thick stratum of soft yellow sandstone; and are underlaid by a deposit consisting of red sandstone, containing in the middle a chocolate-coloured conglomerate, similar to that of the Findhorn. The bold cliffs of the Moray Frith present fine sections of the old red, including the fish beds. The letter is accompanied by illustrative drawings exhibiting the succession, range, and dip of the strata. Mr. Miller gives also an account of a series of faults in the Burn of Ethie, one of which, he conceives, may be traced nearly north to the town of Cromartie.

A paper was read, On the London and Plastic Clay formations of the Isle of Wight, by Mr. Bowerbank, F.G.S.

The object of this communication is to show that there are no zoological distinctions between the London and Plastic Clays. Mr. Bowerbank first examined closely the strata of White Cliff Bay, and found the ascending order of the beds to be as follows:—

Chalk.

1.	Variegated clay, principally red, corresponding with *b* and *c* in the Alum Bay section*....	45 paces.
2.	Dark greenish grey sand, like that of the lowest part of *d*, Alum Bay....................	25 —
3.	Red and yellow sands	27 —
4.	Dark greenish grey sand and clay, similar to *d*, Alum Bay........................	65 —
5.	Red and yellow sands like those of Alum Bay	30 —
6.	Dark greenish gray sand and clay, in which were found *Venericardia planicosta*, Cerithia, and other London clay fossils............	30 —
7.	Variegated sands	6 —
8.	Dark greenish gray sand and clay.......... At different points in this interval the author found small Nummulites, with London clay species of Venus, Voluta, Cerithia, &c., and in one place large Nummulites like those obtained at Bricklesome	186 —

* See Mr. Webster's section in Sir Henry Englefield's Isle of Wight, Geol. Trans., 1st series, vol. ii., Pl. 11.

Chalk. Bay, Sussex, associated with *Venericardia planicosta*, and other London clay shells.

9.	Variegated sands	10 paces.
10.	Dark greenish gray sand and clay like No. 8.	54 —
11.	Variegated sands like those of Alum Bay....	38 —
12.	Greenish gray, brown, and greenish brown clays. This bed contains lignite, sharks' teeth, *Voluta luctator*, Ostrea, and numerous other shells characteristic of the London Clay.	13 —
13.	Yellowish sandy clay, without fossils	26 —
14.	Greenish sand similar to that of the upper marine in Colwell Bay, and containing apparently the same Venus.	12 —
15.	Yellowish sand without fossils	14 —

Beyond this point, freshwater beds, enclosing abundance of Potamides, are displayed.

The above section proves, in Mr. Bowerbank's opinion, that in White Cliff Bay there is an alternation of London and plastic clays throughout 525 paces, and that London clay fossils not only occur abundantly in the part which corresponds with the great mass of that formation in Alum Bay, but are likewise found in the beds, Nos. 8. and 6, which occur below it.

Mr. Bowerbank then described the strata in Alum Bay, taking Mr. Webster's section as the base of his observations; and he pointed out, that in the beds of greenish gray sand and clay marked ***d*** in that section, and below the variegated sand and clays which underlie the London clay, he found the following shells, characteristic of that formation:—*Venericardia planicosta, Cardita margaritacea, Mya intermedia, Cardium semigranulatum, Nucula similis, N. amygdaloides, Turritella conoidea, T. elongata, T. edita, Murex innexus,* (Brander) *Buccinum desertum,* and *Cancer Leachii.* In the variegated sands and clays no fossils were found.

An extract from a letter, dated Newcastle, 14th February, 1839, and addressed to Dr. Buckland, by Mr. Atkinson, was then read.

This letter accompanied a series of slabs of fissile or slaty micaceous sandstone, presenting the tortuous casts of vermiform bodies, either impressed in the stone or in relief. The more perfect casts are marked by a longitudinal line, and closely-set transverse fine striæ. The bed from which the slabs were procured, belongs to the carboniferous formation near Haltwhistle in Northumberland.

The following is the succession of strata presented by the quarry:

Compact sandstone	
Red marly sandstone, with shells	10 inches.
Micaceous blue and white sandstone, containing the casts, the largest of which are found near the centre of the bed. The stone splits into thin flags, and is used for roofing	18 feet.
Compact sandstone	30 —
Limestone containing in one part a few encrinital remains	30 —

The strata dip $15\frac{1}{4}°$ to the S.S.W.

Mr. Atkinson is of opinion that the impressions are principally due to worm-tracks.

A paper was afterwards read, "On the relative ages of the tertiary deposits commonly called Crag, in Norfolk and Suffolk," by Charles Lyell, Esq., V.P.G.S.

This paper contains the results of Mr. Lyell's examination of the crag, with reference to the three following points:—First, The direct superposition of the red to the coralline crag, as originally pointed out by Mr. Charlesworth in 1835: Secondly, Whether the remains of mammalia are really imbedded in regular and undisturbed marine strata in the Norwich crag: Thirdly, Whether the proportion of recent shells, as compared to the extinct, is decidedly larger in the crag of Norwich, so as to indicate a posteriority in age relatively to the Suffolk crag.

1. Of the superposition of the red on the coralline crag, the author found distinct proofs in the sections at Ramsholt and Tattingstone, as previously indicated by Mr. Charlesworth, and in quarries near Sudburne pointed out to him by Mr. Bunbury. At Tattingstone the coralline crag consists chiefly of greenish marl, with discontinuous layers of stone, and the number of corals is very small; but both at that locality and Ramsholt, the red crag rests on denuded beds of the coralline. At Sutton, near Woodbridge, Mr. Lyell was enabled to ascertain, by the assistance of Mr. W. Colchester, that the red crag in some places abuts against a vertical face of the coralline, as well as overlies it; and that in consequence of the irregularities in the outline of the face, the two deposits have a deceptive appearance of alternating. He also ascertained, in addition to the above evidence, that the older or lower strata must have acquired a certain consistency before the newer were accumulated, because the calcareous sand or comminuted shells and zoophytes, of which the former are composed, is perforated to the depth of 6 or 8 feet from the surface by the tortuous borings of pholades, the shells of which are frequently found at the bottom of the tubes, the remainder of the perforations being filled with the sand of the superjacent red crag. The most northern point to which the coralline crag has been traced, is Sizewell Gap, several miles north of Thorpe.

2. With respect to remains of mammalia being imbedded in undisturbed marine beds in the Norwich crag, Mr. Lyell stated, that an examination of this crag in the neighbourhood of Southwold and Norwich had convinced him, that instead of the deposit being purely marine, it is fluvio-marine, containing every where an intermixture of land, freshwater, and sea-shells, with the bones of mammalia and fishes. The formation is exposed along the coast, at Thorpe, near Aldborough, where it may be seen at low-water resting on the coralline crag; but it is most largely developed in the neighbourhood of Southwold, where the author examined it accompanied by Capt. Alexander. In that district, it varies greatly in character, consisting of irregular beds of sand, shingle, loam, and

laminated clay; but it appears to have been in some places tranquilly accumulated, as specimens of *Nucula Cobboldiæ, Tellina obliqua,* and *Mya arenaria,* occur with the valves united, and not worn by attrition. In the same beds, however, are procured rolled fish-bones, and remains of the elephant, rhinoceros, horse, and deer. Capt. Alexander found at the base of the cliff, in a bed about 6 inches thick and rich in marine shells, the tooth of a horse within a large *Fusus striatus*. That gentleman also possesses a tooth of a mastodon, washed out of the cliffs between Dunwich and Sizewell.

In tracing the Norwich crag from Easter Bavant northward towards Kessingland, Mr. Lyell found in it layers of flinty shingle; and he consequently refers to this formation, those strata of sand and shingle, on the coast, which resemble the sandy portions of the plastic clay of the London and Hampshire basins.

In some of the inland pits of Norwich crag near Southwold, the author found mammiferous remains associated with a variety of *Cyrena trigonalis,* a shell common in the freshwater deposit of Grays, and elsewhere.

In the neighbourhood of Norwich the deposit forms patches of very variable thickness, resting upon chalk, and covered by a dense bed of gravel. It is best displayed at Bramerton, Whitlingham, Thorpe, and Postwick, and consists of sand, loam, and gravel, enclosing marine, land, and freshwater shells, with ichthyolites and bones of mammalia; and Mr. Lyell says, it was evidently accumulated near the mouth of a river. The late Mr. Woodward describes the chalk of Postwick as having been drilled by marine animals before the deposition of the crag; and the Rev. Mr. Clowes found in a perforation in the chalk at Whitlingham the shell of a *Pholas crispatus,* the remainder of the perforation being filled with crag. Among other proofs that the strata were gradually deposited, the author mentioned Capt. Alexander's discovery of an elephant's tusk, with many serpulæ attached to it; and he infers from this fossil, that the remains of the mammalia were really washed into the sea of the Norwich crag, and were not subsequently introduced by diluvial action, as some observers have suspected. The freshwater shells, although most diligently searched for, are less abundant than marine, and the terrestrial are still more rare; but Mr. Wigham has found in one bed at Thorpe, a great predominance of fluviatile testacea. In the same pits he obtained a mastodon's tooth at the bottom of the deposit, near the chalk, associated with pectens and other marine shells. In the beds at Postwick, he also discovered, in 1835, part of the left side of the upper jaw of a mastodon, containing the second true molar. This fragment Mr. Owen has been able to identify with the *Mastodon longirostris* of Eppelsheim. In the same bed, Mr. Wigham also obtained the teeth and jaw of a field-mouse, larger than those of the common species; likewise remains of birds, and several species of fishes. The horns of stags, bones and teeth of the horse, pig, elephant, and other quadrupeds, have been obtained at Postwick, Thorpe, Bramerton, &c., near

Norwich; and this association of remains of the mastodon and horse, both in Norfolk and on the continents of Europe and America, Mr. Owen considers as a subject not without interest.

Mr. Lyell examined also the crag north of Norwich at several pits between that city and Horstead, and ascertained that it was of the same kind, resting upon chalk, and overlaid by gravel. He found in it *Fusus striatus, Turritella terebra, Cerithium punctatum, Pectunculus variabilis, Tellina obliqua, T. calcarea, Cardium edule,* and *Cyprina vulgaris*.

3. On the third point, the relative antiquity of the Norwich to the Suffolk crag, and the degree of resemblance of its shells to those of existing series, the memoir contains much very valuable information. The author acknowledges his obligations for assistance during his researches, to Mr. J. B. Wigham, who has nearly doubled the number of Norwich species of testacea; to Mr. Searles Wood, who gave Mr. Lyell free access to his fine collection of crag fossils; and to Mr. G. Sowerby, for the careful comparison and determination of the recent species; he also acknowledges the aid afforded him by Mr. Fitch of Norwich, and Capt. Alexander of Southwold.

The total number of species in the Norwich crag, rejecting those varieties formerly considered to be distinct species, is 111, of which 19 belong to land or freshwater genera. This comparatively small number of species, whether compared with the testacea of the British seas or the Fauna of the Suffolk crag, and not due to want of activity on the part of collectors, or a paucity of specimens, Mr. Lyell explained by showing, that in seas, the water of which is only brackish, as that of the Baltic, or any great estuary, species are far less numerous than in the salt sea, latitude, climate, and other conditions being the same. A similar scarcity of species exists also in the fluvio-marine deposits along the Rhine, between Basle and Mayence.

Of the 92 marine shells of the Norwich crag, Mr. Wood has recognised 73 species found in the red crag, and therefore it might be inferred that the two formations are nearly of the same age; but on applying the test of the proportions of recent species, Mr. Lyell ascertained that the Norwich crag, both with respect to the marine and the freshwater shells, contains between 50 and 60 per cent., whereas in the red crag there are only 30 per cent., and in the coralline but 19.

Mr. Charlesworth had previously implied that the Norwich beds were the most recent, by stating his belief that shells had been washed out of the red crag into the Norwich; and both he and Mr. S. Wood had recognised in the Norwich beds a nearer approach to the existing British Fauna.

The only known freshwater testacea of the red crag of Suffolk were collected by Mr. S. Wood at Sutton, and consist of three specimens of *Auricula myosetis* and one of the variety of *Planorbis marginatus*, with a slightly prominent keel: both of these shells occur in the Norwich crag. Among the other freshwater species of the

Norwich crag is the *Cyrena trigonalis*, found also at Southwold and Crostwick. The land shells consist of *Helix hispida*, *H. plebium*, and a species found at Southwold by Capt. Alexander, bearing a strong resemblance to *Helix Touronensis*, so common in the faluns of Touraine. All the 92 marine species, except two or three, are found either in the red crag or living, so that a very small number were peculiar to this period. It is important to notice, that a large proportion of the recent shells in the coralline crag have not been met with in red or Norwich; but this absence Mr. Lyell attributes to the fragile nature of many of these shells, and in some cases to their having been peculiar to deep or tranquil seas.

In determining the above results, the utmost care was taken to exclude all those shells which might have been washed out of the red crag into the Norfolk, or did not live in the waters by which the latter was deposited.

Should these numerical conclusions hereafter require some modification, still the Norwich crag will be referable to the older Pliocene period, and the red and coralline to different parts of the Miocene.

From an equally careful examination by the author, Mr. Wood, and Mr. G. Sowerby, of the testacea obtained in the superficial lacustrine or fluviatile deposits at Cromer and Mundesley in Norfolk, Stutton, Grays, Ilford, and other places near London, it appears, that the proportion of recent shells in those accumulations is still greater than in the Norwich crag, exceeding 90 per cent., and, consequently, that they must be placed among the newer Pliocene strata.

In a paper communicated to the British Association at Bristol in 1835, Mr. Charlesworth adopted a similar chronological arrangement of the formations above the London clay in the eastern counties, placing the coralline crag at the bottom of the series, the red crag next in ascending order, then the Norwich (mammaliferous) crag, and, highest, the lacustrine strata. In that paper Mr. Charlesworth states, that the proportion of recent to extinct species had not then been determined; and Mr. Lyell remarks, it is satisfactory to find, that the palæontological test of age, derived from the relative approach to the recent Fauna, is perfectly in accordance with the independent evidence drawn from superposition and the included fragments of older beds.

The memoir contains also a general comparison of the fossils of the crag with those of the faluns of Touraine. When M. Desnoyers, in 1825, assigned a contemporaneous origin to both these formations, Mr. Lyell dissented from the conclusion, 1st. because the per-centage of recent species then ascribed to the crag, and determined chiefly from fossils of the Norwich beds, was greater than that of the Touraine deposit; and, 2ndly, because the fossils are not only almost entirely of distinct species, though only 300 miles distant from each other, but that the Fauna of the crag has a northern aspect, and that of Touraine an almost tropical character. A recent examination, by Mr. S. Wood, of a series of Touraine shells procured from

M. Desjardin by Mr. Lyell, has proved, that there are not 10 per cent. of species identical with shells of the crag; but an examination of the same series by Mr. G. Sowerby and the author has led to the conclusion, that the recent species are in the proportion of 26 per cent. Mr. Lyell, therefore, now accedes to the opinion of M. Desnoyers, that the red and coralline crag may correspond in age, generally, with the faluns of Touraine; and he is of opinion that the difference in the character of the two Faunas may be explained by there having existed at that epoch, a more equable climate, similar to the one experienced at present on the east coast of South America, where, in lat. 39°, occur, in a living state, a large Oliva, a Voluta, and a Terebra; and that a geographical barrier, like that of the Isthmus of Suez, which separates the widely different Faunas of the Mediterranean and the Red Sea, may have intervened between the region of the crag and the faluns of Touraine.

The paper concludes with a list of the testacea of the Norwich crag, determined by the author, Mr. S. Wood, and Mr. G. Sowerby*.

LXI. *Intelligence and Miscellaneous Articles.*

PRESENCE OF IODINE IN COAL FORMATIONS.

M. BUSSY states that on examining some mineral specimens obtained from a coal mine in a state of combustion at Commentry (Allier), he ascertained the presence of ammonia and iodine. Having at first observed indications of iodine in some of the specimens, he was afterwards unable to detect it. The iodine was in the state of hydriodate of ammonia, and the acid had left the alkali. As to the origin of this element, M. Bussy supposes that it exists in the bowels of the earth in the state of iodide of potassium, and that it is disengaged in the state of vapour by subterraneous heat. It is well known that ammonia is one of the constant products of the distillation of coal; but the presence of iodine in coal formations is a new fact, worthy of being noticed.—*L'Institut, July,* 1839.

PREPARATION OF CHLOROSALTS. BY M. FILHOL.

When chloride of iodine and an alkaline chloride are mixed, a chlorosalt is formed; this is proved by the following reactions:—

1st. If a mixture of iodide of potassium and chlorate of potash be treated with hydro-chloric acid, the chlorine, iodine, and potassium, simultaneously come into contact; and when the solution is finished, the potassium chlorosalt is obtained on cooling; it may however happen that an excess of chlorine may prevent regular crystallization; very beautiful specimens of the salt may however be obtained.

2nd. If instead of the above-described process there be made a

* The memoir is printed in the Magazine of Natural History for July, 1839. [Mr. Charlesworth's papers, referred to in Mr. Lyell's memoir, will be found in Lond. and Edinb. Phil. Mag. vol. vii. p. 81, 464; viii. 529; x. 1. Edit.]

mixture of iodine and chlorate of potash, and this be treated with hydrochloric acid, assisted with a gentle heat, the chlorate is decomposed, the iodine is totally dissolved, and the resulting chloride of iodine combining with the alkaline chloride, formed at the same moment, constitutes the same chlorosalt; by operating in this method, it is easier to avoid an excess of chloride of potassium, which frequently entirely prevents crystallization.

3rd. If iodine be added to a solution of potash, and a current of chlorine be passed through it, chloride of potassium and chloride of iodine are formed, and the prismatic crystals of the chlorosalt soon appear in the liquor; care must be taken that the quantity of iodine is sufficient, in order to form the salt, for, as already stated, an excess of the alkaline chloride greatly prevents crystallization.

4th. The formation of the potassium chlorosalt is still more readily effected if a current of chlorine be passed into a concentrated solution of chloride of potassium, mixed with iodine; in this case, the alkaline chloride, being ready formed, no more chlorine is required than is requisite to saturate the iodine.

5th. If there be made a mixture of pure, solid iodic acid, with hydrochloric acid, these two acids, as is well known, decompose each other, with the disengagement of chlorine and the formation of chloride of iodine; these solutions, incompletely saturated with potash, soon deposit prismatic needles of a fine golden yellow colour; in this case, it is the same salt which is formed; but there is generally precipitated at the same time some iodate of potash.

It is evident that these chlorosalts may be formed under various circumstances, and probably others may be readily found besides those now pointed out; the simplest is that of mixing the two chlorides.

The conclusions which M. Filhol arrives at from the experiments contained in the memoir, of which the foregoing is an extract, are:

1st. That concentrated hydrochloric acid completely decomposes the iodates into chloride of iodine, and chloride of the base of the iodate, with the disengagement of chlorine.

2. That weak hydrochloric acid decomposes iodate of potash, forming bi-iodate of potash and chloride of potassium.

3. That the chloride of iodine formed in the first case, is the chloride I^2Cl^6, that is to say the actual perchloride.

4. That the chlorides of potassium, magnesium, and hydrochlorate of ammonia, combine with chloride of iodine, and form true chlorosalts.

5. That these chlorosalts are formed of an equivalent of each of the two elements.

6. That alkaline iodides are decomposed by chlorine, and may, under the long-continued action of a current of this gas, give rise to the same chlorosalts.

7. That the salt resulting from the saturation of the chloride of iodine, dissolved in hydrochloric acid by potash, is still the same chlorosalt and contains no iodate.

8. That the salt resulting from the saturation of chloride of iodine

dissolved in water by potash, is the salt described by Sérullas under the name of chlor-iodate of potash, and composed of bi-iodate of potash and chloride of potassium.—*Journal de Pharmacie*, August, 1839.

CHLORO-STANNITE OF MERCURY.

M. Capitaine gives this name to the white matter which sublimes when chloride of tin is formed, by the combination of the protochlorides of tin and of mercury.

The best process for preparing it is the following: take 24 parts of protochloride of mercury, and 3 parts of tin amalgamated with a portion of the mercury. These quantities correspond to rather less than two equivalents of the protochloride to one equivalent of tin. These substances after being thoroughly mixed are to be put into a retort capable of holding four times the quantity, on account of the swelling up which occurs when the action begins; when the retort is heated by a few coals, the action soon takes place with a slight noise, the cessation of which shows that the action is complete. When the retort is cold it is to be broken, and it is found to contain a grey substance, which is frequently spongy; and beneath this there is some mercury, which separates during the action. This grey matter is to be pulverized and put into a flat-bottomed matrass, which is to be heated to about the boiling point of mercury. Bichloride of tin is vaporized, and the chloro-stannite of mercury sublimes, and there remain in the matrass anhydrous protochloride of tin, and some mercury.

When white vapours cease to arise in notable quantity, the operation is to be stopped; when the matrass has cooled, it is to be cut at about an inch from the bottom, and the upper part will be found studded with white dendritic crystals of chloro-stannite of mercury; these are to be removed with feather, and kept in a well-stopped bottle, for they attract moisture from the air.

The properties of this substance are, that it is perfectly white. It is volatile, but its point of volatilization is simultaneous with that of its point of decomposition, so that it cannot be volatilized unchanged. When it is heated a portion only sublimes, the remainder is decomposed into metallic mercury, protochloride of tin, and perchloride of tin, which may be collected.

When this substance is treated with water, it becomes grey, and then black; this happens because the protochloride of mercury is reduced by the protochloride of tin.

In analysing this substance the determination of the mercury is readily effected; it is necessary only to take a known weight of it, and to boil it for a short time with water acidulated with hydrochloric acid, to which it is better to add a little protochloride of tin, in order to be sure that the whole of the mercury is reduced to the metallic state; the chlorine and tin are easily determined: when the salt is treated with carbonate of soda, there are produced oxide of tin, oxide of mercury, and chloride of sodium; when the whole is thrown on a filter, the liquor passes through, at first clear, but if the filter be washed with pure water, oxide of tin passes through with it. In order to prevent this, the filter, after the solution has

run through it, must be dried without washing, in the funnel; the oxide of tin contracts in drying, is partly peroxidized, and eventually loses its property if passing through the filter with water; the washings are then to be added to the first liquor, and the whole, acidulated by nitric acid, is precipitated by nitrate of silver. It is very important to boil the matter for some time with the carbonate of soda; without this precaution, the decomposition would be imperfect, and there would be a loss of chlorine.

To determine the tin, the filter containing the oxides of mercury and tin is to be dried; it is then to be treated with pure nitric acid, in a porcelain crucible, and this is then to be heated till the filter is perfectly burnt. The crucible is then to be weighed, and the tin is determined by the weight of the stannic acid produced.

One hundred parts yielded

Mercury	60·31
Tin	17·68
Chlorine	21·09
	100·08

The results correspond to the following constitution:

Mercury..	2 atoms ..	2531·65 or	60·97
Tin......	1 atom ..	735·29 ..	17·71
Chlorine..	4 atoms ..	885·30 ..	20·31
		4152·24	100·

Journal de Pharmacie, September, 1839.

[The equivalent of mercury being taken as 202, that of tin 58, and of chlorine 36, it will be seen that this compound consists of

One equivalent of protochloride of mercury....	238
One equivalent of protochloride of tin.........	94
	332.

R. P.]

ANHYDROUS PROTOCHLORIDE OF TIN.

M. Capitaine gives the following as a simple and very œconomical method of preparing anhydrous protochloride of tin: Heat the muriate of tin of commerce in a large Hessian crucible; it is not possible to perform the operation in a retort, as some authors advise, for the salt swells and passes over into the receiver without undergoing any alteration; when moderately heated in the crucible it fuses, swells and yields the vapour of water and of hydrochloric acid; soon after this it undergoes igneous fusion, and when in quiet fusion it is to be poured into a smaller crucible. On cooling it becomes a brown mass, which is to be reduced to coarse powder and distilled in a coated glass retort. The first product is perfectly pure, the latter contains a little iron, which is to be separated by redistillation. The properties of this substance are that it is compact, nearly of a pure white colour, brittle, and has a vitreous fracture; when heated it fuses at about 482° Fahr., boils, and volatilizes at a little below a red heat. It possesses a property which must always prevent the density of its vapour from being ascertained, which is that it is never completely volatilized without undergoing partial decomposition, the

results of which are perchloride of tin, and yellow, earthy subchloride of tin, which remains in the distilling apparatus. When in a state of fusion it has a great tendency to penetrate the crucibles.

It dissolves in water without undergoing any immediate change; but the liquor remains clear only for a short time, and soon becoming turbid, it deposits oxychloride of tin. After a considerable time, in contact with the air, the oxychloride dissolves and the liquor becomes clear and transparent.

Absolute alcohol dissolves it very readily, and more when hot than cold; by cooling, the solution becomes syrupy. After some time the solution acquires a distinct odour of hydrochloric æther. When exposed to the air it long retains its property of precipitating the chloride of gold purple, and it does not become turbid. The protochloride of tin when anhydrous, is much less alterable by exposure to the air than when it contains water; indeed unless it be hydrated it does not appear to absorb oxygen; after three weeks' exposure it remains completely soluble in water.—*Journal de Pharmacie*, September, 1839.

METEOROLOGICAL OBSERVATIONS FOR SEPT., 1839.

Chiswick.—Sept. 1. Cloudy: boisterous. 2. Showery. 3. Fine: rain: fine at night, with splendid Aurora borealis. 4. Very fine: rain. 5. Rain. 6. Very fine: rain. 7. Fine: cloudy: very boisterous: almost a hurricane at night. 8. Fine. 9. Hazy: very fine. 10. Foggy: fine. 11, 12. Overcast. 13. Clear and fine. 14. Heavy rain. 15. Showery. 16. Rain. 17. Very heavy rain. 18. Showery. 19. Overcast: clear. 20. Very fine. 21. Rain: cloudy: rain at night. 23. Foggy. 24. Cloudy. 25. Showery. 26. Very fine. 27. Cloudy and fine: slight rain. 28. Drizzly. 29. Heavy dew: fine. 30. Fine.

The quantity of rain which fell in this month, amounting to little short of 4 inches, may be reckoned as double an average quantity.

Boston.—Sept. 1. Stormy: rain A.M. 2. Fine: rain A.M. 3. Cloudy: rain A.M. and P.M. 4. Fine. 5. Cloudy: rain early A.M.: rain P.M. 6. Fine. 7. Stormy: rain P.M. 8. Stormy: rain early A.M. 9. Cloudy. 10. Fine. 11. Cloudy: rain early A.M. 12. Cloudy: rain A.M. 13. Fine: rain P.M. 14. Cloudy: rain early A.M. 15, 16. Cloudy: rain early A.M.: rain P.M. 17, 18. Fine: rain early A.M. 19. Fine: rain early P.M. 20. Fine. 21—23. Misty. 24, 25. Cloudy. 26, 27. Fine. 28. Cloudy: rain early A.M.: rain P.M. 29, 30. Fine.

Applegarth Manse, Dumfries-shire.—Sept. 1. Heavy showers all day. 2. Occasional showers. 3. Warm and close. 4. Occasional showers, with gleams of sunshine. 5. Unceasing rain. 6. Dull but fair: shower in evening. 7. Almost constant rain: flood. 8. Rain began at noon: all day. 9. Rain greater part of the day. 10. Very fine day: pleasant breeze. 11. Fine day, but cloudy. 12. Fair till noon, when it rained. 13. Showery. 14. Stormy: wind and rain all day. 15. Moderate, with frequent showers. 16. Fair till afternoon, when it rained. 17. Showery, with intervals fair. 18. Showery all day. 19, 20. Fine A.M.: showery P.M. 21. Fair and calm: distant thunder. 22. Fine harvest day: slight shower P.M. 23. The same: no rain. 24. Fine A.M.: rain P.M. 25. Heavy rain morning: cleared up. 26. Frequent showers. 27, 28. Continued succession of heavy showers. 29. Fine harvest day: one slight shower. 30. Calm: dry and sunny.

Sun 25 days. Rain 24 days. Thunder 1 day.

Wind south 11 days. South-east 5 days. South-west 3½ days. West 5 days. West-north-west 1 day. East 3 days. North ½ day. Variable 1 day.

Calm 8 days. Moderate 11 days. Brisk 3 days. Variable 1 day. Strong breeze 5 days. Boisterous 2 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. Sept.	Barometer. London: Roy. Soc. 9 a.m.	Barometer. Chiswick. Max.	Barometer. Chiswick. Min.	Barometer. Boston. 8½ a.m.	Barometer. Dumfries-shire. 9 a.m.	Barometer. Dumfries-shire. 8½ p.m.	Thermometer. London: Roy. Soc. Fahr. 9 a.m.	Thermometer. London: Roy. Soc. Self-register. Max.	Thermometer. London: Roy. Soc. Self-register. Min.	Thermometer. Chiswick. Max.	Thermometer. Chiswick. Min.	Thermometer. Boston. 8½ a.m.	Thermometer. Dumfries-shire. Max.	Thermometer. Dumfries-shire. Min.	Wind. London: Roy. Soc. 9 a.m.	Wind. Chiswick 1 p.m.	Wind. Bost.	Wind. Dumfries-shire.	Rain. London: Roy. Soc. 9 a.m.	Rain. Chiswick.	Rain. Boston.	Rain. Dumfries-shire.	Dew point. Lond.: Roy. Soc. 9 a.m.
1.	29·072	29·185	29·061	28·49	28·68	28·70	62·0	63·5	56·6	68	50	61	59½	51	sw.var.	sw.	w.	s.	·180	·01	·42	...	60
2.	29·220	29·314	29·194	28·66	28·83	28·95	59·8	64·8	53·0	66	50	57	61	48	wsw.	sw.	w.	se.	·008	·11	·12	...	53
3.	29·450	29·634	29·440	28·85	29·14	29·30	57·7	63·5	52·0	67	46	58	59½	43½	sw.var.	sw.	w.	s.	·122	·01	·16	...	55
4.	29·764	29·962	29·745	29·18	29·54	29·69	57·5	61·0	50·0	71	53	60	61	45½	w.	w.	w.	sw.	·041	·19	·15	...	54
5.	29·918	29·910	29·885	29·33	29·55	29·55	62·4	66·0	57·0	70	53	58	59	48½	w.	sw.	w.	s.	·133	·06	·06	...	58
6.	30·032	30·067	29·998	29·40	29·67	29·67	61·8	67·0	56·0	74	46	60	62	52	w.	w.	w.	s.	·061	·04	·02	1·85	58
● 7.	29·916	29·918	29·785	29·32	29·46	29·27	62·5	68·0	55·0	66	55	60·5	57	52½	s.	sw.	sw.	s.	·011	...	...	...	57
8.	29·978	30·006	29·936	29·33	29·60	29·55	60·8	65·7	56·2	71	61	60	58½	49	sw.var.	sw.	w.	s.	·030	...	·11	...	56
9.	30·046	30·026	29·971	29·38	29·53	29·60	65·0	68·0	61·0	75	51	65	59	55½	sw.	sw.	sw.	w.	...	...	...	...	60
10.	30·030	30·131	29·999	29·40	29·79	29·95	62·4	69·5	57·0	78	59	60·5	60	49	w.	sw.	w.	w.	...	...	...	...	60
11.	30·048	30·047	29·878	29·47	29·89	29·72	62·2	70·0	60·5	69	58	60	58	46	se.	s.	calm	sse.	...	·04	·04	...	62
12.	29·804	29·804	29·726	29·22	29·65	29·50	61·0	69·8	60·0	63	41	58	59½	42	nw.	w.	calm	w.	·019	...	·05	...	59
13.	29·672	29·669	29·429	29·11	29·38	29·30	57·5	63·0	49·0	65	50	54	56	44	sw.	s.	nw.	w.	...	·09	·03	...	54
14.	29·166	29·165	29·120	28·75	29·14	29·10	58·6	63·5	53·5	66	50	56	55	41½	se.	se.	s.	e.	·013	·84	·14	2·68	59
15.	29·148	29·288	29·134	28·72	29·07	29·07	58·5	60·9	55·0	68	50	59	62	52	s.	s.	se.	e.	·205	·20	·10	...	58
☽ 16.	29·368	29·382	29·356	28·89	29·10	29·14	58·5	62·5	53·5	65	51	58	59½	47	s.	s.	s.	e.	·105	·21	·26	...	55
17.	29·554	29·536	29·467	28·98	29·12	29·19	58·2	62·5	53·6	62	46	58	57½	51	sw.	s.		sw.	·102	·90	·10	...	55
18.	29·530	29·556	29·511	28·98	29·16	29·20	58·0	60·8	49·0	67	46	54	57	48½	sw.	sw.	w.	s.	·691	·52	·15	...	55
19.	29·518	29·494	29·465	29·03	29·25	29·30	58·5	60·0	51·0	64	48	53	55½	47½	ssw.	s.	calm	s.	·672	·08	·11	...	53
20.	29·626	29·621	29·611	29·13	29·34	29·41	58·3	60·9	51·0	67	49	54	56	44½	ssw.	sw.	s.	sse.	·036	·04	·01	...	53
21.	29·528	29·631	29·548	29·10	29·44	29·41	51·7	61·7	51·9	63	42	51	59	45½	nw.	n.	calm	ssw.	·102	·30	...	1·81	52
22.	29·628	29·773	29·620	29·16	29·48	29·55	55·4	58·7	50·0	64	41	50	58	36	sse.	s.	calm	s.	·336	·17	...	...	52
○ 23.	29·784	29·877	29·765	29·30	29·62	29·65	52·5	60·3	46·0	67	38	47·5	57	35	sw.	sw.	calm	sw&n	·044	...	...	...	50
24.	29·894	29·885	29·798	29·40	29·60	29·47	54·6	59·2	49·0	66	54	52	60½	42	s.	s.	calm	s.	...	·06	...	...	50
25.	29·768	29·841	29·749	29·16	29·32	29·44	60·3	60·0	54·0	71	48	59	58½	43½	ssw.	s.	s.	s.	·055	·03	...	...	57
26.	29·856	29·886	29·839	29·30	29·48	29·58	59·0	64·5	55·0	66	42	55	58	44	wsw.	s.	w.	w.	·016	...	...	...	55
27.	29·930	29·917	29·693	29·32	29·58	29·34	55·0	63·0	48·5	64	49	54	53½	46	ssw.	sw.	w.	sse.	...	·01	...	...	52
28.	29·498	29·528	29·474	29·03	29·27	29·29	56·0	60·5	54·0	65	36	57	55	49	s.	s.	s.	se.	...	·01	·02	1·30	55
☾ 29.	29·642	29·980	29·625	29·16	29·38	29·55	52·6	60·4	48·0	66	36	51	56½	46	wsw.	sw.	calm	wnw.	...	...	·11	...	50
30.	29·856	29·979	29·832	29·40	29·75	29·85	51·5	60·0	45·0	67	41	51	56½	38	e.	e.	calm	Var.	...	...	...	...	49
Mean.	29·675	29·733	29·621	29·13	29·392	29·409	58·3	63·5	53·0	67·36	48·06	56·3	58·1	46·1					Sum. 2·982	3·92	2·16	7·64	Mean. 55·2

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[THIRD SERIES.]

DECEMBER 1839.

LXII. *On the Combinations of Carbon with Silicon and Iron and other metals, forming the different species of Cast Iron, Steel, and Malleable Iron.* *By* Dr. C. Schafhaeutl, *of Munich**.

THE opinions of the generality of chemists, concerning the chemical constitution of carbon, are briefly as follows:

The *diamond* is carbon in its highest state of purity. Dr. Thomson considers next to the diamond the carbon obtained from decomposed coal-gas.

Next in order may be considered *graphite* or *plumbago*, anthracite, coke from stone-coal, charcoal, pit-coal, and animal coal.

Sir Humphry Davy considers the difference between the diamond and common charcoal to consist only in the form of aggregation of their molecules; but he has at the same time shown that diamond burnt in oxygen gas produced nothing but carbonic acid gas, while charcoal always left behind *traces of water*, notwithstanding it had been previously exposed to the highest degrees of temperature.

These and other experiments caused Berzelius, in the earlier editions of his "Treatise on Chemistry," to explain, that the difference between the diamond and charcoal and other coaly matters, arose from the combination of carbon in the latter with other substances; but having, in all probability, neither the time nor the inclination to follow up the matter, he abandoned it in the last edition of his treatise, asserting,

* Communicated by the Author: an extract from this paper was read on the 31st of August last before the Section of Chemistry and Mineralogy of the British Association at the meeting at Birmingham.

that the difference between the diamond and charcoal consisted only in their form of aggregation.

Dr. S. Brown closed his highly interesting paper, read at the late Birmingham Meeting, "*On the Crystallization of Carburets*," with the following remarks: "Char wood with sufficient care, and you will obtain, not charcoal, but crystallized carbon, or in other words, *diamonds*."

When Scheele first burnt graphite or plumbago, which had hitherto always been confounded with molybdena, oxide of iron remained on the hearth, and graphite was declared to be a carburet of iron. Berthier first showed that the iron in the graphite might be extracted by means of acids without any evolution of hydrogen; Bouesnel and Karsten, burning graphite in the muffle of an assay furnace, observed, that several of the natural graphites, such as that from Bareros, in the Brazils, left an extremely small quantity of ashes. Graphite was therefore declared to be only another modification of pure carbon.

The greatest support the above theory received was from the fact, first observed by Gahn, that some pieces of charcoal, which had descended unconsumed through a blast furnace, and fallen into the twyers, had totally changed their nature, and when taken out, immediately became extinguished; and being before extremely porous, became very dense, and from one of the worst conductors of caloric, they had become nearly one of the best.

In order to investigate and elucidate the before-mentioned facts, I prepared carbon in as pure a state as possible, first, by decomposing dry carbonic acid gas by means of potassium; secondly, by imperfect or partial combustion of alcohol; and thirdly, by distilling white crystallized sugar in a retort. The three charcoals thus obtained were inclosed in as many tubes of platinum, the ends of which were, after being first ignited to a white heat, then hermetically welded, and afterwards placed in a tube made of Stourbridge clay, filled also with charcoal, and heated in a blast furnace of Sefstroem till the tubes began to melt. After refrigeration, these different samples of charcoal were found perfectly unaltered, both in their physical and chemical properties. The same experiment was repeated by including the same descriptions of charcoals in crucibles made of Stourbridge clay; and during a repetition of these experiments for sixteen times, in *two instances* only did the charcoal alter its nature. By burning these two samples in oxygen gas, as well as with alkaline carbonates, a large quantity of silica and alumina, with traces of iron, remained, and showed that the *charcoal* had extracted

these substances in the process from *the clay of the melting-pots.*

These pure charcoals, which I analysed, I found, both before and after their exposure to the highest degree of temperature, invariably to consist as follows:

1st, Of carbon, hydrogen, and traces of potassium not extractable by acids.

2ndly, Carbon, hydrogen, nitrogen; and these charcoals never parted from their hydrogen, &c., unless they had the opportunity of combining with silicon or aluminum or iron instead.

Graphite or plumbago is sometimes an artificial product, and separated from the iron on the hearth of the blast furnace in the process of making gray *kishy* cast iron, that is to say, by using a very easily fusible mixture of limestone and iron ore, and keeping up a great heat in the furnace. This artificial plumbago is called by the workmen *kish* *. Good white charcoal iron, which is for sometime kept in a high degree of temperature, and afterwards very slowly cooled, is invariably converted into gray iron; so, in like manner, gray charcoal iron may be converted by sudden cooling into white.

As the generation of kish and gray iron never takes place, except during the *hot working* of the blast furnace, the conclusion was drawn by Karsten that graphite or kish could only be generated in the highest degrees of temperature; and he was led by this circumstance to infer, that by treating iron ores in small crucibles with charcoal, white iron was always obtained, and very seldom gray iron.

I shall, however, show that the generation of graphite commences at 1500 degrees Fahr., measured with Daniell's pyrometer, a degree in which even brass remains in its rigid state.

In some works where gray cast iron or pig iron is immediately converted into malleable iron, without undergoing first the process of refining, the hearth of the puddling furnace remains half filled with a kind of liquid slag, which consists principally of a trisilicate of iron and manganese. In this liquid mass the raw pig iron is converted, during a rapid effervescence of the whole, into malleable iron, and the slags, after the operation, run off into small cast iron boxes, over the bottoms of which is spread coal-dust, to prevent the slag from adhering to the bottom and sides of the boxes. As soon as this slag runs over this coal-dust, a vivid evolution of

* [On this subject see a paper by Mr. (now Professor) E. Davy, Phil. Mag. First Series, vol. xl. p. 41, 44.—Edit.]

gas takes place, and on cooling, the bottom of this slag, not only that part which has touched the coal, but also where it came in contact with the gas evolved, is found to be covered with a thinner or thicker pellicle of graphite or plumbago, which possesses all the properties, both externally and chemically, of the kish or graphite, which separates itself from gray kishy iron during its refrigeration.

The larger fragments of the coal enveloped by the liquid slag are either converted into a very light black spongy body, retaining the outlines of the coal fragment, and perfectly enveloped with an interlayer of a *silvery rugose pellicle of graphite*; or the coal itself is converted into a metallic shining body, perfectly resembling coke, in which case *no graphite pellicle* is to be found. This last circumstance appears only to occur when the silicate is at the highest degree of temperature. For the generation of this kind of artificial graphite, a very liquid silicate is invariably necessary, as well as highly bituminous coal, the temperature ranging between 1500 and 2000 degrees Fahr., and probably much lower, as I shall presently show. I saw sometime since in the iron works of Messrs. Solly and Sons, near Dudley, thin streams or layers of slag running out of the flue hole of a balling or reheating furnace, and moving slowly towards an already cooler cake-like mass of the same slag. Just before the *running slag* came in contact with the other already stiff one, I put some coal-dust in its way, over which the wave ran, immediately overlapping the already cooler ore. The heat of the liquid slag, measured by the expansion of platinum, was 1500° Fahr. The usual gases escaped as a yellowish brown smoke, and on their cooling, on inspecting the two slightly adhering pieces of slag, I found the lower one, just over the point of connexion, where the smoke escaped, surrounded by a lively *orange-red border*, probably an oxide of iron, which could not be removed from the surface. By tracing this, and separating carefully the pieces from each other, I found the lower cake of slag covered on its rounded edges with the before-mentioned *graphite pellicle*, forming *near the orange-coloured borders one inseparable and continuous body with the slag itself*, but getting gradually *looser* as it descended towards the *bottom of the cake*. I cut the pellicle with a knife across from the bottom of the cake, and it rolled itself immediately up towards the top, where it immersed itself into the mass of the slag, so that even with a magnifying glass it could not be distinguished from the slag itself. The pellicle was near the bottom about the thickness of common letter-paper, but gradually becoming thinner, until its trace became lost on the surface. The *surface* of the slag,

where the graphite pellicle originated, was likewise *altered*; the surface of the common puddling or balling slag is of a dull blueish or of a somewhat slaty appearance, originating in the closely interwoven angles of the crystals, which constitute the mass of the slag. But in this case the *whole space of the slag* covered with the pellicle of graphite, presented a *glassy shining appearance*, resembling dark green bottle glass; and viewed through a microscope had a velvety appearance, that is to say, was formed of small points with circular intervals, which caused an interference of the rays of light, and was therefore coloured with the usual bands originating under such circumstances.

The manner in which this graphite pellicle was formed, shows distinctly that not only *carbonaceous matters* are required for generating graphite, but that a *silicate of iron and manganese* is indispensably necessary; for the reason, that no other substance is capable of forming graphite with coaly matter; and secondly, principally because the pellicle was not only *mechanically an interlayer* of crystallized carbon between the two slags, but it *actually rose out of the surface of the slag*, had taken up a portion of the surface, and finally lost and immerged itself entirely in the surface: but at the same time highly bituminous coal is necessary.

The coal of Rive de Gier, near Lyons, in France, produces readily and plentifully pellicles of graphite; the coals of Alais, Pont du Gard, on the foot of the Cevennes, which contain very little bitumen, produced only an inseparable powdery or scaly layer of plumbago or graphite.

The graphite produced by the before-mentioned means must be divided into two different species.

Graphite (*a*), generated about the thickness of *paper*, always consists of elastic leaves of a rather *dull blueish appearance*, similar to that of annealed sheets of iron.

Graphite (*b*), about the thickness of *gold-leaf*, is shiny, extremely light, and to the touch similar to natural plumbago.

All species of plumbago are considered unalterable in all kinds of acids; but I found that concentrated hydrofluoric acid changes their nature entirely, and converts all descriptions of plumbago which have hitherto come under my observation, into a flocky light dull carbonaceous mass, during the escape of fluo-silicic acid gas.

Plumbago has of late been burnt in the muffle of an assay furnace, in order to separate the foreign non-volatile bodies. This mode of procedure is ineffective, as the current of air and the rising carbonic gas carry away with them the great-

est part of the remaining finely divided, and therefore, extremely light silica.

Nitrate of potash is likewise not to be recommended for decomposing plumbago, as the deflagration is so violent, that not only much of the mass is liable to be lost, but a great deal of it remains undecomposed; and the silver crucible very frequently melts. I found carbonate of potash or soda decomposing plumbago quietly and perfectly, even in a dull red heat. If a white heat be used, the evolution of carbonic acid gas is very violent; so much so, that drops of the liquid mass are thrown up against the lid of the crucible, and the crucible itself is very much attacked. In order to ascertain the volatile parts of plumbago, it must be mixed with chromate of lead and chlorate of potassa in a glass tube heated over a coal fire, and the gas collected after the well-known methods of Berzelius and Liebig.

Before ascertaining the proximate chemical components of black lead, I digested the well-washed and cleansed material in nitromuriatic acid for several days in a sand bath; the solution was then separated from the black lead, evaporated to dryness, afterwards dissolved in hydrochloric acid, the remaining silica collected on a filter, and the oxide of iron thrown down by ammonia; the well-washed residuum of black lead was then, still wet, heated with a strong lye of caustic potash, or better, soda, the filtered lye neutralized with hydrochloric acid, evaporated to dryness, and the silica separated in the well-known manner.

Graphite (*a*) in paper-like leaves or scales, appeared after this treatment to have perhaps increased slightly in brightness, but graphite (*b*) remained quite unaltered in its appearance. The *graphite*, freed in this manner from its mechanical admixtures, was then mixed with five times its weight of carbonate of soda, and kept for half an hour in a bright red heat; and after the crucible had been allowed to cool, the alkaline mass was found perfectly fused, with a dirty yellowish brown centre, when graphite (*a*) had been used, but perfectly white when graphite (*b*) was subjected to the same process. The chemical composition of (*b*) was by this method found to be in 100 parts

(*a.*) Substances soluble in acids and alkalies.

Protoxide of iron	18·60
Silica	7·62
	26·22

Very nearly resembling the formula $\dot{F}^3\,\dddot{S}$.

(*b.*) The remaining 73·78 parts of graphite consisted of

Carbon	70·3421
Silicon	3·0744
Loss	00·3635
	73·7800

and corresponds to 4·16 per cent. silicon.

As it was impossible to extract the silica with caustic and carbonaceous lyes, even after a week's digestion, and as the silicon cannot be considered as silica without our obtaining an improbable surplus in weight, we may safely conclude, that silicon existed as *such*, in the graphite, and in chemical combination with it; therefore we may assume graphite to be a *supercarburet of silicon*, in which about 86 atoms of carbon are combined with one atom of silicon.

Graphite (*b*) gave a dissimilar result. It retained, even after the longest treatment with acids and alkalies, its first form of elastic dull leaves of considerable thickness, and was found to consist in 100 parts of

Silicon	4·93
Iron	9·50
Carbon	85·45
Loss	00·12
	100·00

It is scarcely necessary to remark that the relation of the silicon to the iron when combined with oxygen in graphite (*b*) very nearly resembles the formula $\dot{F}^3\,\dddot{S}^2$, that is, a sesquisilicate of iron resembling the composition of puddling slag; and that graphite (*a*) may be considered as a compound of carburet of iron with the carburet of silicon.

This curious generation of graphite, only by the interference of silicon and manganese, from the volatile matters of coal, seems to afford us some hints towards explaining the origin of graphite in blast furnaces, which is in all probability formed in like manner, particularly as I observed that graphite is never generated in melting pots from pure iron and pure carbon, except when the black oxide of the surface began to attack the sides of the crucible.

That graphite makes its appearance in blast furnaces in the highest degrees of heat, is not that it is only generated in this highest degree of heat, but that it cannot exist without a quantity of *alumina* being first reduced and combined with the iron.

The volatile parts of coal, and particularly tar, a combination of pyrretin with ammonia, exerts its reducing power over

the silicates of the slag by depositing a great quantity of finely divided coal, as is sometimes the case in gas retorts.

If we assume the puddling slag to consist of a mixture of different silicates, from which the sesquisilicate of iron very often separates itself during the process of cooling, we may easily consider that one atom of the silica of the sesquisilicate is reduced to silicon, combining with the superabundant carbonaceous matter, in the nascent state, into a carburet of silicon, which with the other three atoms of protoxide of iron and one atom of silica, remains mechanically mixed; and, indeed, when we consider the before-mentioned parts of graphite (*b*) extractable by acids and alkalies, we come very near the formula $\dot{F}^3 \dddot{S}$ (*Silicias triferrosus*).

On the contrary, with graphite (*a*), both constituents of the slag, oxide of iron and silica, appear to be reduced, and combining with carbon into supercarburets; and these two different chemical compounds are easily explained by the different degrees of heat by which they are generated, and the *length of time* allowed for their formation.

These two singular and differing species of graphite appear to be the prototypes of the two different species of cast iron, as is shown in the following tables.

Graphite (*b*.).		Gray Cast Iron.	
Iron.... Silicon ..	Oxygen, Silicate of iron.	Iron .... Silicon...... Aluminum .	Silicet and aluminet of iron.
Silicon .. Carbon ..	Carburet of silicon.	Carbon...... Silicon......	Carburet of silicon.
Graphite (*a*).		**White Cast Iron.**	
Iron .... Carbon ..	Carburet of iron.	Iron.........	Carburet of iron.
		Carbon .... Azote	Cyanuret of iron ?)
Carbon .. Silicon ..	Carburet of silicon.	Silicon...... Carbon ...	Carburet of Silicon.

The molecules of all sorts of iron, the elementary parts of which are perceptible by the aid of a magnifying glass, belong to the cubical system, generally of equal size, of which the largest grain never exceeds 0·0000633 of an inch; their form appears to be always the same, and their different modes of aggregation in the various sorts of iron is all that can mechanically form the difference between cast and malleable iron, as far as ocular inspection, assisted by magnifying glasses, can penetrate.

Iron in which those molecules are aggregated in the most perfect *crystalline form*, having all their faces in *one plane of crystallization*, is called *perfect dark-grey iron.*

Some tendency of the molecules to crystallize by their ar-

rangement in nodal aggregations begins to be developed in tilted but softened best *cast steel* and in white cast iron; but in their most perfectly equal division throughout the whole mass of the iron, without any certain relative position of their surfaces, they appear in tilted but *hardened cast steel of the highest conversion,* and for their looser and more irregular, and in large masses fibrous arrangements, are the different species of *malleable iron,* the fairest specimens.

Some time since graphite was considered mechanically mixed with the mass of steel; and in consequence of this theory, it was asserted by many writers on the subject, that the graphite scales might be seen in the fractures of steel and intimately intermixed with it, even with the naked eye. Now it is perfectly well known that no such scales can be discovered in any steel whatever; but in gray cast iron its peculiar appearance is still explained to be derived from the scales of graphite, notwithstanding the impossibility of discovering it by the highest magnifying power.

The glittering scale-like particles which appear in the fracture of gray cast iron are, in fact, the sides or faces of a crystalline figure, and those perfectly flat sides are divided regularly by their higher crystalline orders, into trapeziums or pentagons, the diameter of the most regular being about 0·000355 of an inch, those of the greatest length being about three times their width; and those pentagons are again found to be composed of the above-mentioned molecules of only 0·0000633 of an inch diameter; but no scales which could interrupt the regular arrangement of those molecules can be perceived.

A fragment of such very gray cast iron under a microscope resembles much a fragment of some anthracite of South Wales, both as regards its fracture and colour; and the forms of imperfect prisms are never to be mistaken in the whole formation.

In the cross fracture of hardened cast steel (of a razor forged in my presence in the factory of Messrs. Rogers of Sheffield) it appeared milky white, somewhat like deadened silver. By the aid of a microscope the molecules appeared equally distributed, and their facettes *never* in one plane, so that the whole fracture, in the field of a microscope, appears to be composed of innumerable bright points, the plane sometimes interrupted by elongated furrows, in which may be discovered the angles of deeper situated points, the whole having a somewhat similar appearance to the full moon when first viewed through a common telescope.

I took the other parts of this fractured razor, heated it care-

fully to a light red heat, and suffered it to cool slowly. Being then broken again, the milk-white earthy fracture was now changed to a more distinct granulated, and a more blueish metallic surface. Under the microscope this surface had lost its *equality* of the disposition of the molecules; several molecules had now aggregated together, having their bright faces all in one plane, and approaching in form the facettes of gray iron. The whole surface of the fracture was spread over with such nests or bundles of crystals, and the furrows between them appeared to be much widened.

These striking differences between hardened and soft cast steel appear to me to explain at once the great hardness of cast steel dipped red-hot into water, and its property of not being acted upon by the file.

It is well known that the power of mutual attraction between molecules is almost irresistible, and it follows that when once the equilibrium of a liquid mass is destroyed different centres of attraction are forming themselves, towards which the nearest molecules move, grouping themselves around it, and assuming a geometrical form according to their original shape. In this crystalline form the molecules are in a state of equilibrium, and their equilibrium is stable in both respects; but the power of cohesion acting from different centres, and in certain directions, it is the greatest only in the central points of crystals and approaching to the axis of crystallization, and the mutual attraction or cohesion of those crystalline bodies thus formed is infinitely weaker than the internal central cohesive force of the molecules, which constitute those crystalline aggregates. So in a somewhat similar manner, the hardest of all bodies, the diamond, is very easy to be cleaved parallel to the faces of the primary form of its crystal.

Now when caloric enters into such a crystallized body, the relative distance between the molecules increases, the stability of the equilibrium in respect to position is decreasing; therefore the form of the molecules begins to lose its influence, in the same ratio as the equilibrium in respect of distance preponderates, thus dividing and acting on the molecules with *perfect equality throughout the whole mass.*

Suppose now we abstract suddenly the caloric before the relative attractive force of the molecules has time to arrange them perfectly in an equilibrium of position; all the irresistible powers of attraction are revived and restored: the molecules will therefore attract each other with equal force throughout the whole mass, and consequently there appears no reason why any body, formed of the same material, and under the same equally powerful influences of equal molecular

attraction, should be able to destroy the molecular cohesion of another body before its own molecular attraction of equal force is overcome.

The colour of the different sorts of iron seems to depend for the most part on the closer, looser, or the more or less regular arrangement of their molecules, as the molecules of all the different descriptions of iron when viewed by means of the microscope present an equally bright appearance.

Nevertheless we must be cautious in drawing our conclusions from appearances under high magnifying powers. Microscopes seem to have a power of penetrating space as well as Herschel's telescopes; at least, the same piece of razor, the fresh fracture of which I had exposed to a dull red heat, had considerably lost the bright colour of its fracture; nevertheless, viewed through the microscope, the molecules appeared to retain their accustomed brightness, and could not be distinguished from those of a fresh fractured piece when placed by the side of it. Further, upon exposing the same piece to a higher degree of temperature, till the fracture was covered with a skin of oxydul impenetrable to the naked eye, yet viewed through the microscope the whole granulated texture was distinctly visible, and only a slight nebulous veil appeared to cover a little of its original brightness.

The more or less compact arrangement of the molecules is expressed by the specific gravity of the iron, but in ascertaining the specific gravity of them, it must be always borne in mind, that cast iron in large lumps has, from the surface to the centre, not only a varying degree of density, but very often also a different chemical composition. I ascertain, therefore, the specific gravity on a large scale by weighing a fragment of an entire section of a cast-iron pig, or by taking out of the liquid iron a small but deep ladleful of metal, the specific gravity of which was ascertained after slowly cooling.

The specific gravity of thirty-five different sorts of iron which I analysed with all possible care are the following, reduced to 61° F., and 30 inches height of barometer.

Specific Gravity for 61° F., and 30 inches height of Barometer.

Boilings (puddlings), slag adhering during working to the stirring tools	3·773
Boilings, slag towards the end of the puddling process	4·519
Slag forming the bottom of the puddling furnaces	4·6109

Iron.

1. Gray cast iron from Vienne (France)	6·898
2. English bar iron heated in a smith's forge	6·949
3. Grayish iron from Yorkshire	6·954
4. Lumps of iron (?) which remained on the bottom of crucible in which iron with lamp-black had been melted (crystallized)	6·998

5.	Dead gray metal from Terre Noire, near Lyons	7·014
6.	Grayish iron from Tollend, S. Staffordshire	7·144
7.	Mottled iron from Staffordshire	7·329
8.	White metal forced out from cast-iron pigs at Alais	7·339
9.	Whitish gray cast iron from Alais	7·404
10.	White cast iron from the Marley Iron-works, South Wales	7·407
11.	White cast iron from Alais, (fine granulated)	7·442
12.	Mottled iron from Staffordshire	7·451
13.	White crystalline iron	7·472
14.	Bar iron heated in a reheating furnace	7·487
15.	Wootz from Bombay	7·508
16.	No. 12. as plate metal	7·513
17.	Cement steel made from English iron (highest concentration)	7·519
18.	Cement steel from Swedish iron (double bullet) middling hard	7·563
19.	Cast iron, silvery white, extremely large-grained, from Alais	7·582
20.	Cement steel from Swedish iron (*Koop L.*), highest conversion	7·621
21.	White-charcoal iron from Fourchambault (France)	7·709
22.	Cold-short bar iron from Staffordshire	7·753
23.	Cement steel made from bar iron, prepared from Staffordshire iron according to my method of puddling (highest conversion)	7·755
24.	Swedish iron called *Koop L.* in soft gray leaves	7·761
25.	Iron melted by me with 3 per cent. of sugar coal (it hardened not at all)	7·764
26.	Swedish iron, called double bullet, fracture fine-granulated	7·810
27.	Bar iron, according to my puddling process for steel (fine-granulated)	7·841
28.	The same, the granulations interrupted by fibres	7·845
29.	The same, the fibres more developed	7·840
30.	No. 23. in cast-steel ingots	7·892
31.	The foregoing tilted into small bars for razors	7·900

Of the lowest specific gravity, it will be seen, was gray brilliant cast iron from Vienne, department d'Isere in France, equal to 6·898; and of the greatest specific gravity appears tilted razor-steel, prepared, according to my method, from English iron at Mr. Solly's works at Dudley, equal to 7·900.

[To be continued.]

LXIII. *Memoir on Rational Derivation from Equations of Coexistence, that is to say, a new and extended Theory of Elimination. By* J. J. Sylvester, *F.R.S., Professor of Natural Philosophy in University College, London**. (Part I.)

Any number of equations existing at the same time and having the same quantities repeated, may be termed equations of coexistence: in the *present* paper we consider only the case of *two* algebraical equations:

$$x^m + a_1\, x^{m-1} + a_2 x^{m-2} + \ldots\ldots + a_m = 0$$
$$x^n + b_1\, x^{n-1} + b_2 x^{n-2} + \ldots\ldots + b_n = 0$$

* Communicated by the Author.

The above being "equations of coexistence," x is called "the repeating term."

If we suppose the equation

$$c_0 x^r + c_1 x^{r-1} + c_2 x^{r-2} + \dots\dots + c_r = 0$$

to be capable of being deduced from the two above, and, therefore, necessarily implied by them, this will be called "a Particular Derivative" from the equations of coexistence of the r^{th} degree, (r being supposed less than m and n *, and the coefficients being *rational* functions of the coefficients of the equations of coexistence.)

There will be an *indefinite* number in general of such derivatives, and the form involving arbitrary quantities which includes them *all* is called "the general derivative of the r^{th} degree."

Any "Particular Derivative," in which the terms are all integral, numerically as well as literally speaking, is called an "Integral derivative."

That "Integral Derivative" of any given degree in which the literal parts of the coefficients are of the *lowest possible dimensions* †, and the numerical parts as low as they can be made, is called the "Prime Derivative" of that degree. So that there is nothing left ambiguous in the prime derivative save the sign.

The "Derivative by succession" is that particular derivative which is obtained by performing upon the equations of coexistence, the process commonly employed for the discovery of the greatest common measure and equating the *successive remainders* to zero.

To express the product of the sums formed by adding each of one row of quantities to each of another row, we simply write the one row above the other; a notation clearly capable of extension to any number of rows, which would not be the case if we spoke of *differences* instead of *sums* ‡.

Theorem (1.)

Let $h_1, h_2, \dots h_m$, be the roots of one equation of coexistence,

* This restriction upon the value of r is not essentially requisite, and is only introduced to keep the attention fixed upon the particular objects of this 1st Part.

† Of course the dimensions of the coefficients in the equations of coexistence are to be understood as denoted by the indices subscribed.

‡ The wider views which I have attained since writing the above, and which will be developed in a future paper, lead me to request that this notation may be considered only as temporary. It would have been more in accordance with these views to have used the two rows to denote products of differences than of sums. But a change now in the text would be very apt to cause errors in printing.

$k_1, k_2, \ldots k_n$, the roots of the other. The general derivative of the r^{th} degree is represented by

$$\Sigma \left(\mathrm{SR}\,(h_1\, h_2\, h_3 \ldots h_r)\, \{\overline{x-h_1} \cdot \overline{x-h_2} \ldots \overline{x-h_r}\} \times \left\{ \begin{matrix} h_{r+1}\, h_{r+2} \ldots h_m \\ -k_1\, -k_2 \ldots -k_n \end{matrix} \right\} \right) = 0$$

SR $(h_1 \,.\, h_2 \,.\, h_3 \ldots h_r)$ denoting any *symmetrical rational* (integral or fractional) function of $h_1\, h_2 \ldots h_r$;

$\left\{ \begin{matrix} h_{r+1} \,.\, h_{r+2} \ldots h_m \\ -k_1 \,.\, -k_2 \ldots -k_n \end{matrix} \right\}$ being to be interpreted as above explained, and Σ of course including as many terms as there are ways of putting n things r and r together*.

A form tantamount to the above, and which may be substituted for it is its analogue,

$$\Sigma \left(\mathrm{SR}\,(k_1 \,.\, k_2 \ldots k_r) \,.\, \{\overline{x-k_1} \,.\, \overline{x-k_2} \ldots \overline{x-k_r}\} \times \left\{ \begin{matrix} k_{r+1} \,.\, k_{r+2} \ldots k_n \\ -h_1 \,.\, -h_2 \ldots -h_m \end{matrix} \right\} \right) = 0$$

When $r = 0$ the theorem gives simply

$\left\{ \begin{matrix} h_1\, h_2 \ldots h_m \\ -k_1 - k_2 \ldots - k_u \end{matrix} \right\} = 0$ and is coincident with that given by Bezout in his Theory of *Elimination*.

Subsidiary theorem (A).

If $h_1\, h_2 \ldots h_m$ be the roots of the equation

$$x^m + a_1 \,.\, x^{m-1} + a_2\, x^{m-2} + \ldots\ldots + a_m = 0$$

and if $e^m + a_1\, e^{m-1} + a_2\, e^{m-2} + \ldots\ldots + a_m - u = 0$ then

$$\Sigma \frac{h_1^{\,r}}{(h_1-h_2) \cdot (h_1-h_3) \ldots (h_1-h_m)} = \frac{1}{r+1} \cdot \frac{d}{d\,u} \cdot \Sigma\,(e^{r+1}),\, u$$

being made zero after differentiation.

Cor. If R h_1 denote any integral rational function of h_1,

then $\Sigma \dfrac{\mathrm{R}\,(h_1)}{(h_1-h_2)\,(h_1-h_3) \ldots (h_1-h_m)}$ is *always* integral and is *zero* when the dimensions of R (h) fall short of $(m-1.)$

* The general derivative may clearly be expressed also by the sum of any two particular derivatives affected respectively with arbitrary rational coefficients. The equivalency of an arbitrary *function* to two arbitrary *multipliers* is very remarkable, and analogous to what occurs in the solution of certain differential equations.

Subsidiary theorem (B).

$$\Sigma \frac{\mathrm{SR}\,(h_1\,h_2\,\ldots\,h_r)}{\begin{Bmatrix} h_1\,.\,h_2\,\ldots\,h_r & \\ -h_{r+1}\,.\,-h_{r+2}\,\ldots & -\,h_m \end{Bmatrix}}$$

can be expressed by the sum of terms, each of which is the product of series of the form

$$\Sigma \frac{\mathrm{R}\,(h_1)}{(h_1 - h_2)\,.\,(h_1 - h_3)\,\ldots\,(h_1 - h_m)}$$

it is *always* integral, and when the dimensions of the numerator fall short of $(m - r)\,r$ it vanishes*.

Subsidiary theorem (C).

The *only* modes of satisfying the equation

$$\Sigma \left(f\,(h_1\,.\,h_2\,\ldots\,h_r) \times \mathrm{SR}\,(h_1\,.\,h_2\,\ldots\,h_r)\right) = 0$$

for all forms of the latter factors short of $\overline{m-r}\,.\,\overline{n-r}$ dimensions, is to put $f\,(h_1\,h_2\,\ldots\,h_r) = 0$, or else

$$f\,(h_1\,h_2\,\ldots\,h_r) = \frac{\text{constant}}{\begin{pmatrix} h_1\,h_2\,\ldots\,h_r & \\ -h_{r+1}\,.\,-h_{r+2}\,\ldots & -\,h_m \end{pmatrix}}$$

Theorem (2.)

By virtue of the subsidiary theorem (B), the two equations

$$\pm\,\Sigma \left((x - h_1)\,(x - h_2)\,\ldots\,(x - h_r) \times \frac{\begin{Bmatrix} h_{r+1}\,.\,h_{r+2}\,\ldots & h_m \\ -k_1\,.\,-k_2\,\ldots & -k_n \end{Bmatrix}}{\begin{Bmatrix} h_{r+1}\,.\,h_{r+2}\,\ldots\,\ldots & h_m \\ -h_1\,.\,-h_2\,\ldots & -h_n \end{Bmatrix}} \right) = 0$$

$$\pm\,\Sigma \left((x - k_1)\,(x - k_2)\,\ldots\,(x - k_r) \times \frac{\begin{Bmatrix} k_{r+1}\,.\,k_{r+2}\,\ldots & k_n \\ -h_1\,.\,-h_2\,\ldots & -h_m \end{Bmatrix}}{\begin{Bmatrix} k_{r+1}\,.\,k_{r+2}\,\ldots\,\ldots & k_n \\ -k_1\,.\,k_2\,\ldots\,\ldots & -\,k_r \end{Bmatrix}} \right)\ 0$$

are each integer derivatives of the r^{th} degree.

* It may be remarked also in passing, that any term in the numerator which contains *any* one power not greater than $m - 2\,r$ may be neglected and thrown out of calculation. Moreover, an analogous proposition may be stated of fractions in the denominators of which *any number* of rows are written one under the other: see the last note to page 429.

Theorem (3.)

And by virtue of the subsidiary theorem (C), the above two equations are the "Prime Integer Derivatives," and are exactly identical with each other.

Cor. 1. The leading coefficient of the "prime derivative" of the r^{th} degree is always of $(m-r)$ $(n-r)$ dimensions.

Cor. 2. If P_r be called the prime derivative of the r^{th} degree and if $(X=0\ Y=0)$ be the two equations of coexistence, and λ_r μ_r the two "prime constituents of multiplication" to the said derivative, *i. e.* if λ_r and μ_r satisfy the equation $\lambda_r . X + \mu_r Y = P_r$, then the coefficient of the leading terms in λ_r and in μ_r is of $(m-r-1)$ $(n-r-1)$ dimensions.

Theorem (4.)

The "Prime Derivative" of any given degree is an exact factor of the "derivative by succession," of the same degree. The quotient resulting from striking out this factor is called "the quotient of succession."

Theorem (5.)

If L_1 L_2 L_3, &c. be the leading coefficients of the derivatives occurring first, second, third, &c., in order after the equations of coexistence, and if Q_1 Q_2 Q_3, &c. represent the first, second, third, "quotients of succession" reckoned in the same order, then

$$Q_1 = 1$$

$$Q_2 = \frac{1}{L_1^2}$$

$$Q_3 = \frac{L^4_1}{L_2^2}$$

$$Q_4 = \frac{L^4_2}{L^4_1 . L^2_3}$$

and in general

$$Q_{2n} = \frac{L^4_2 . L^4_4 \ldots L^4_{2n-3} . L^4_{2n-2}}{L^4_1 . L^4_3 \ldots L^4_{2n-1} . L^2_{2n-1}} *$$

$$Q_{2n+1} = \frac{L^4_1 . L^4_3 \ldots\ldots L^4_{2n-3} . L^4_{2n-1}}{L^4_2 . L^4_4 \ldots\ldots L^4_{2n-2} . L^2_{2n}} \dagger.$$

* That the appearance of the index 4 may not startle, let my reader bear in mind that there are what may be termed secondary derivatives of succession for every degree appearing in the process of successive division.

† The prime derivatives must be capable of yielding an *internal* evidence of the truth of Sturm's theorem. In fact, for the case of all the roots be-

Cor. Hence, in place of Sturm's auxiliary functions, we may substitute the functions derived from the equations of coexistence $\left(fx=0, \frac{d\,fx}{d\,x}=0\right)$ according to theorem (2.) due regard being had to the sign.

Scholium. Hitherto it has been supposed that the values of the coefficients in the equations of coexistence are independent of one another, but particular relations may be supposed to exist which shall cause the leading terms given by theorem (2.) to vanish, giving rise to anormal or singular primes, as they may be called, of the degree r of fewer than $(m-r)$. $(n-r)$ dimensions. The theory of this, the failing case, (so to say) is highly interesting, and I have already discovered the law of formation for the quotients of succession on the supposition of *any number* of primes vanishing consecutively; but I forbear to vex the patience of my reader further, the more so, as I hope soon to be able to present a complete memoir, with all the steps here indicated filled up, and numerous important additions, (the perfect image of which this is but a rough mould,) as homage to the learned and illustrious society which has lately done me the honour of admitting me into its ranks.

Why this has not already been done must be excused, by the fact of the theory having suggested itself abroad in the intervals of sickness*. Yet thus much will I add in general terms, viz. that as many primes as vanish consecutively, so many units must be added to the index 2 of the accessions received in the numerator and denominator of the subsequent quotient; and in the quotient after that, it is not the square of the leading term of the penultimate prime,—but the product of this

ing possible, a little consideration will serve to show that the leading term of each prime derivative of the $=n \left\{fx \; \frac{d\,fx}{d\,x}\right\}=0$ will consist of a series of fractions, each of which fractions is, *numerically* speaking, of the *same sign*.

* The reflections which Sturm's memorable theorem had originally excited, were revived by happening to be present at a sitting of the French Institute, where a letter was read from the Minister of Public Instruction, requesting an opinion upon the expediency of forming tables of elimination between two equations as high as the 5th or 6th degree containing one repeating term. The offer was rejected, on the ground of the excessive labour that would be required. I think that this has been very much overrated; and probably many will be of the same opinion who have dwelt upon the fact that no numerical quantity will occur in the result higher than the highest index of the repeating term. Would it not redound to the honour of British science that some painstaking ingenious person should gird himself to the task? and would not this be a proper object to meet with encouragement from the Scientific Association of Great Britain?

term by the leading term of that anormal prime of the same degree which has the lowest dimensions,—that finds its way into the numerator. The rest of the formation remaining undisturbed, *unless* and *until* a new failure have taken place.

Note on Sturm's Theorem.

When one of the equations of coexistence is the differential coefficient with respect to the repeated term of the other, the prime derivatives given in theorem (2.) which coincide in this case with Sturm's auxiliary functions *reduced* to their lowest terms, may be exhibited under an integral *aspect*.

Let SPD intimate that the squared product of the differences is to be taken of the quantities which follow it.

Let S_1 indicate the sum of the quantities to which it is prefixed.

S_2 the sum of the binary products.

S_3 the sum of the ternary products, and so on.

Let $h_1\ h_2 \ldots h_n$ be the roots of any equation.

Then Sturm's last auxiliary function may be *replaced* by SPD $(h_1\ h_2 \ldots h_n)$.

The last but one may be replaced by

$\Sigma\,\mathrm{SPD}\,(h_1 h_2 \ldots h_{n-1})\,x + \Sigma\,S_1\,(h_2 h_3 \ldots h_{n-1})\ \mathrm{SPD}\,(h_1\, h_2 \ldots h^{n-1})$.

The one preceding by

$\Sigma\mathrm{SPD}\,(h_1\ h_2 \ldots h_{n-2})\,x^2 + \Sigma\ S_1 (h_1 h_2 \ldots h_{n-2})\,\mathrm{SPD}\,(h_1 h_2 \ldots h_{n-2})\,x$
$+\Sigma\ S_2\ (h_1\ h_2 \ldots h_{n-2})\ \mathrm{SPD}\ (h_1\ h_2 \ldots h_{n-2})$ and so on.

Thus then Sturm's rule for determining the absolute number of real roots in an equation is based wholly and solely upon the following

ALGEBRAICAL PROPOSITION.

If there be n quantities, real and imaginary, the imaginary ones entering in pairs, as many changes of sign as there are in the terms

$$\left.\begin{array}{l} \Sigma\ \mathrm{SPD}\ (h_1\ h_2) \\ \Sigma\ \mathrm{SPD}\ (h_1\ h_2 \ldots h_3) \\ \ldots\quad \ldots\quad \ldots \\ \ldots\quad \ldots\quad \ldots \\ \Sigma\ \mathrm{SPD}\ (h_1\ h_2 \ldots h^{n-1}) \\ \quad \mathrm{SPD}\ (h_1\ h_2 \ldots h_n) \end{array}\right\}\ \text{so many in number are these pairs.}$$

Query (1.) Is there no proposition applicable to any (n) quantities *whatever*?

Query (2.) Is there no faintly analogous proposition applicable to higher powers than the squares?

Query (3.) Seeing that in forming the coefficients in the equation to the squares of the differences, we pass from (n) functions of the roots, to $n\,\frac{n-1}{2}$ and not n functions, of their

squared differences, does not a natural passage to the former lie through n functions of the squared differences?

In other words, may not the quantities Σ SPD $(h_1\, h_2 \ldots h_n)$, &c., serve as natural and valuable intermediaries between the coefficients of an equation involving simple quantities and the coefficients of the equation involving the squares of their differences?

P.S. In the next part I trust to be able to present the readers of this Magazine with a *direct* and *symmetrical* method of eliminating any number of unknown quantities between any number of equations of any degree, by a newly invented process of symbolical multiplication, and the use of *compound* symbols of notation.

I must not omit to state that the constituents of multiplication λ_r and μ_r explained in Cor. 2. to Theorem (3.) are equal to the expressions

$$\Sigma\,(x-k_1)\,(x-k_2)\ldots(x-k_{m-r-1})\frac{\begin{pmatrix} k_1 \,.\, k_2 & \ldots\; k_{m-r-1} \\ -h_1-h_2 & \ldots\; -h_n \end{pmatrix}}{\begin{pmatrix} k_1 \,.\, k_2 & \ldots\; k_{m-r-1} \\ -k_{m-r} & \ldots\; k_m \end{pmatrix}}$$

and its analogue respectively.

University College, London, October 15, 1839.

[To be continued.]

LXIV. *Observations on* Professor Plateau's *Defence of his Theory of accidental Colours. By* Sir David Brewster, *K.H., V.P.R.S. Ed.**

In the Numbers of this Journal for May and June last, Professor Plateau has published an elaborate defence of his "Theory of accidental Colours," against certain observations which have been made upon it in England. The authors to whose observations he replies are an anonymous writer in the Edinburgh Review † and myself; and as I have no hesitation in acknowledging that I am the anonymous reviewer, I feel myself bound to bear the whole burden of examining the able defence which Professor Plateau has now made. Having the honour of being personally acquainted with this distinguished individual, and admiring his character as well as his talents, I regret that my opinion should have been placed in collision with his; but from the temperate and truly philosophical manner in which he has conducted the discussion, and which in so far as the first of these qualities is concerned I shall not fail to imitate, I trust the interests of science and of

* Communicated by the Author.

† April 1834, p. 160.

truth will not be compromised by a further analysis of those phænomena of our visual sensations respecting which these differences of opinion have arisen.

Although I have devoted much time to the study of the phænomena of accidental colours, yet my observations have been made at distant intervals, and recorded from time to time without any design of establishing a particular theory. At one time I believed in the theory of the diminution of sensibility, till a series of specific observations led me to a new theory, which, as I shall afterwards show, differs very little, if it differ at all, from that of M. Plateau in its leading aspect.

To this general theory M. Plateau has added two distinct propositions respecting the *combination of accidental colours*, and their *law of succession*; and it is on these points that I have the misfortune of differing with him in opinion.

On the first of these points M. Plateau maintains, that while "the combination of *real colours* produces *white*, the combination of *accidental colours* produces *the contrary to white*, or *black*." That is, "whereas two real complementary colours produce together *white*, two accidental complementary colours produce together *black* *."

With respect to this proposition, I have stated in the Edinburgh Review, that we cannot, with any propriety of language, consider *one* accidental colour as *added to, or combined with another*; that the proposition itself is a *verbal illusion*; and that the physical fact which it expresses has been long known to philosophers, and is indeed the *necessary* result of our previous knowledge on the subject.

After endeavouring to controvert this opinion, M. Plateau does not conclude that it is erroneous; he concludes only, and he puts his conclusion in *italics*, that he has not *merely expressed what has been long known*. The ground upon which he rests this conclusion is, that I did not seem to be aware that his proposition included the *new fact*, that accidental colours were seen in total obscurity. Now I admit the force of this exception, and I acknowledge that the visibility of accidental colours in the dark was not known to Buffon, Darwin, or Rumford; but this fact has no connexion with the proposition under discussion, any further than that it overturns the theory of sensibility, and weakens my argument in so far as it *was supposed to depend* upon that theory.

Now though I abandoned as incorrect, and have long ago abandoned, the theory of a diminution of sensibility as furnishing the *true explanation* of the phænomena of accidental colours, I still maintain that a diminution of sensibility is a ne-

* *Ann. de Chim.*, Aug. 1833, p. 388, and this Journal, vol. xiv. p. 334.

cessary *accompaniment* of accidental colours; that is, that the portion of the retina which affords the accidental colour is more or less insensible to every other colour but that which it affords. To deny the influence of diminished sensibility as a *cause*, is a very different thing from denying it as an effect, and it is by confounding these two propositions that M. Plateau seems to me to have erred.

Now if we admit, what I think is capable of the most rigorous proof, that there is a diminished sensibility of the part of the retina affected, it follows as a matter of course that the sum of those diminished sensibilities for all the rays of white light must be *an insensibility to them all*, that is *blackness.*

But why should we examine M. Plateau's proposition, by means of *theories*, or of *opinions*, that are in any way disputed or ambiguous? Is it not a demonstrated fact, admitted by every philosopher, that the accidental colour of *white* is *black*? that is, the sum of the actions of all the component colours of white light, when their action has ceased, or the combination of all their accidental colours, is *blackness.* If we take two complementary colours, namely the *red* and *green* tints forming the *ordinary* and *extraordinary* pencils in the polarized ring, which by overlapping form *white light*, then it is manifest that the accidental colour of the overlapping part is *black*, and hence the sum of the action of the *red* and *green* acting separately must also be *black*.

I have no hesitation, therefore, in averring that the view which I have taken in the Edinburgh Review and elsewhere of Prof. Plateau's proposition is in every respect correct.

In replying to the observations of Professor Plateau in defence of his theory of accidental colours, I feel a difficulty of a very peculiar kind. He admits *that his theory is more frequently opposed by the results of experiment* than supported by them*; and as this is the very opinion which I have maintained, it would seem almost unnecessary to continue the discussion. In case, however, I may have misapprehended that part of his reply in which this admission is made, it will be necessary to enter more minutely into the subject.

But before I do this, I must acknowledge a mistake which I have committed, and which he has pointed out, in ascribing to him the assertion, *that the regular alternation of the* PRIMITIVE *and* ACCIDENTAL *colour, is the effect most frequently observed*, whereas he has maintained the *contrary* proposition, *that the effect which he most frequently observed was that of the* DISAPPEARANCE *and* REAPPEARANCE of the negative or accidental impression alone. In expressing my regret for this mistake, into which I was led by believing Professor Plateau's

* In this Journal, vol. xiv. p. 340.

theory to have a better foundation than it has, I must call the reader's attention to the fact, *that my mistake was most unfavourable to my own argument;* and that had I not made the oversight, I should have been able to overthrow Prof. Plateau's theory by means of his experiments as well as by my own.

This theory, as given in § 5, of Prof. Plateau's *Essai d'une Théorie Générale*, &c. &c., Brussels, 1834, is expressed in the following manner: "The retina returns to its primitive state by a series of decreasing oscillations like a *spring* or a *pendulum*; the primitive impression being rapidly effaced in order to be followed by an *opposite* effect, the nature of which will be determined by experiment; the primitive impression will then show itself again, but more feebly, and so on in succession, till the eye cannot distinguish the impressions any longer." The theory therefore is that the PRIMITIVE *and* ACCIDENTAL *colours follow each other in regular succession till they disappear altogether.*

After an elaborate and able examination of the various phænomena of accidental colours, Prof. Plateau gives the following general result, which, though mixed with much hypothetical language, expresses clearly enough the experimental results at which he has arrived:—

"When the retina is submitted to the action of rays of any colour, it resists this action, and tends to resume its ordinary condition with a force more or less intense. If it is then suddenly withdrawn from the exciting cause, it returns to its ordinary condition by an oscillatory movement, the intensity of which is proportional to the duration of the previous action; a movement, in virtue of which the impression passes, at first, from the *positive* to the *negative* state, *then continues generally to oscillate in a manner more or less regular*, while it becomes weaker and weaker. Sometimes it *only disappears and reappears alternately*, and sometimes it passes *successively from the* NEGATIVE *to the* POSITIVE state, and *vice versa*."*

Prof. Plateau goes on to state that the "agreement between the results of experiment and the *second* hypothesis (the hypothesis already given) is remarkable. If the oscillations of the impression which is effaced are *generally incomplete or irregular*, we must attribute it to causes which the actual state of this part of the science of vision does not yet permit us to appreciate."

Without taking advantage of the circumstances that Prof. Plateau does not here acknowledge, as he does in the *Annales de Chimie*†, that the *appearance and disappearance of the accidental colour is the fact most frequently observed*, the admission,

* *Essai*, &c. p. 64. † August, 1833, p. 39

that even the oscillations, when they do occur, are *generally incomplete or irregular*, is a complete proof that the *theory* is not supported by experiment; nay, results directly opposite to the theory are acknowledged to be more frequent than results agreeing with it, and these last are admitted to be generally incomplete or irregular.

Now, Prof. Plateau admits, that the most frequent result, namely, the disappearance and reappearance of the accidental colour, was observed by many authors, and particularly by Scherffer and Darwin. This is perfectly true, and indeed I cannot conceive how any person could have studied the subject experimentally without observing it. With regard to the *rarer* phænomenon which Prof. Plateau and Prof. Quetelet have each seen, and which represents the theory, I have frequently observed the one colour succeed the other, but *never* the whole phænomenon; and since I have read Prof. Plateau's writings, I have endeavoured in vain to see it.

Here then we have two classes of facts; one of which, viz. the *rarer* class, leads to *one* theory, and the other, viz. the more *common* class, leads to another theory; and Prof. Plateau adopts the theory least supported by facts, solely, we presume, because it appears more beautiful, and more consistent with analogy.

Regarding the subject as not ripe for generalization, I have ventured to explain the phænomena of the disappearances and reappearances of the accidental colour, and the occasional succession of the negative and positive impressions, by the operation of certain *disturbing causes*, which must undoubtedly exercise an influence over all such phænomena. Prof. Plateau answers these arguments by stating, that if such causes had presented themselves, "he must have been a very unskilful observer not to have noticed their influence, nor have secured himself against them." But observers not very unskilful have actually found that the accidental colour may be obliterated temporarily or permanently by involuntarily winking, or by closing the eyes with different degrees of pressure, or by distending the eyes, or by knocking the head; and we believe that the results of all such experiments are affected even by the position of the head, the indirect action of light, the state of the stomach, and the pressure of the blood-vessels on the eye-balls.

Before concluding these observations, I must beg leave to correct a mistake, no doubt accidentally made by Prof. Plateau in comparing my theory of accidental colours with his own. "In a certain point of view, as may be seen, Sir David Brewster's theory and mine approach each other; and indeed,

in the first place, they have that in common, that they consider the accidental colour as owing to an impression of a peculiar nature which is spontaneously generated in the organs, and not as the result of a relative insensibility to certain rays. On the other hand, according to Sir David Brewster, the accidental colour unfolds itself on the retina during the contemplation of the direct colour, and combines itself with this latter; and according to me, the *opposite* effort of the retina, whence results the *negative* sensation, as soon as that *effort* ceases to be counteracted, likewise unfolds itself during the contemplation of the direct colour, and combines itself, *in some respect*, with this latter, neutralizing it partially. But only Sir David Brewster maintains *that the combination of the two sensations produces whiteness*, whereas I have shown that upon an object insulated from every lateral influence the result is on the contrary *blackness**."

Now it is not the case, that I maintain "that the combination of the two sensations produces *whiteness*." My words are: "The effect of this vision of the *green* (the accidental colour seen when looking at a *red* seal,) is *to make the* RED *appear much paler by its admixture with it*. The *red* and *green* tend to produce whiteness; but as the *direct red* greatly predominates over the *accidental green*, the result *is always pale red*." Now as the original seal was a *bright red*, and as I maintain that the physiological result is a *pale red*, why does Prof. Plateau allege that I make the combination of the two sensations produce *whiteness*? Does not he himself maintain the *same opinion* which I have above expressed, when he says†, "that the combination of the two actions" is a gradual falling off in the apparent brightness of the object looked at? No observer can doubt the correctness of my opinion, or rather of the *fact* which I observed.

Upon the supposition that his *theory of negative and positive oscillations* is an incontrovertible law of our visual sensations, M. Plateau, like all theorists who are not satisfied with the plain results of experiment, extends it *to all the other senses*, when their respective organs, caused to deviate from their ordinary condition, are suddenly withdrawn from the exciting cause. They return, he conceives, to their ordinary condition by an oscillatory movement, which produces *negative* impressions. He endeavours, without any success, to apply this principle to certain phænomena of *sound* and *touch*, and even taste‡; and, pushing his theory to the very verge of sobriety, he asks, "if it is not probable that the principle of oscillatory

* See this Journal, vol. xiv. p. 443. † *Ibid.* p. 442, bottom.

‡ He considers the *arrière goût*, or *back taste*, as the *negative oscillation*!

movements may be extended to phænomena of the most elevated order, *to facts purely moral*? Who does not know, for example, that the liveliest *joy* is often followed by a sentiment of *sadness*, which, gradually decaying, is again followed by *agreeable recollections*, which are themselves finally effaced? Have we not here *oscillations* decreasing from *pleasure* to *pain*, from *pain* to *pleasure*; and from *pleasure* to the *normal state of the mind*?" Our ingenious author employs the same principle to explain the *effects of moral contrast*; but we shall pursue the subject no further, lest we should be accused of prejudicing our readers against our author's *optics*, by dwelling upon the singularities of his *metaphysics*. We must warn our young readers, however, against being led away from the path of sober inquiry by the pursuit of such ingenious analogies. It may be amusing to fanciful minds, for it certainly is *poetical*, to regard *pleasure* and *pain*, *joys* and *sorrows*, as the *lights* and *shadows* of our being,—as the *positive* and *negative tints* which colour in succession the livid perspective of life; but the principle, if once encouraged, would soon be pushed to a more hazardous extent, and the ever-varying phases of our moral nature would soon be represented by the abscisses and ordinates of a mathematical curve.

St. Leonard's, St. Andrews, Nov. 12, 1839.

LXV. *Observations of Shooting Stars on the nights of the* 10*th and* 11*th of August*, 1839. *By* R. M. Z.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

A REQUEST having been made that any notice relative to the August periodical shooting stars should be communicated to the Philosophical Magazine, I have transmitted the following observations made by me on the 10th and 11th of August inst. Brief and imperfect though these observations may be, yet perhaps, as *facts* bearing upon a hitherto little explored subject, they may not be wholly devoid of interest.

I must be permitted to premise, that my observations were confined to a limited portion of the heavens, the aspect of my apartment being N.N.W. and that I was without an assistant. On the night of the 10th I was prevented from commencing any observations until twenty minutes past eleven, and at thirty minutes past twelve the sky became overcast, and the observations were discontinued. During this time (with,

* On the subject of this communication, see our last Number, p. 372. —Edit.

however, occasional unavoidable interruptions, certainly not leaving a full hour for the observations,) I observed thirty-five shooting stars, fifteen of which were very brilliant; eleven of the latter left luminous trains, usually visible for some seconds, and in most instances characterized by sparks. The wind was at the time blowing very fresh from the south-west, and the direction of the shooting stars was uniformly quite contrary, that is, from between the north and east towards the opposite quarter of the sky. One of the more brilliant shooting stars was apparently lost behind a cloud. A faint flash of reddish coloured lightning was also observed.

On the night of the 11th, the sky was enveloped in clouds until towards midnight: the clouds were remarkably luminous at about eleven o'clock. At fifty minutes past eleven, the sky was partially visible, and I observed a brilliant shooting star of a reddish hue. At twelve the clouds were much dispersed, and between that time and thirty minutes past twelve, I observed seven shooting stars, none of which were of great brilliancy, and only one of which left a luminous train: the direction of the latter (which was in the constellation of Ursa Major) was remarkable, being perpendicular; that of the others was similar to the course of the shooting stars observed on the preceding night.

I am, Gentlemen, yours, &c.

R. M. Z.

Clapham, Surrey, Aug. 14, 1839.

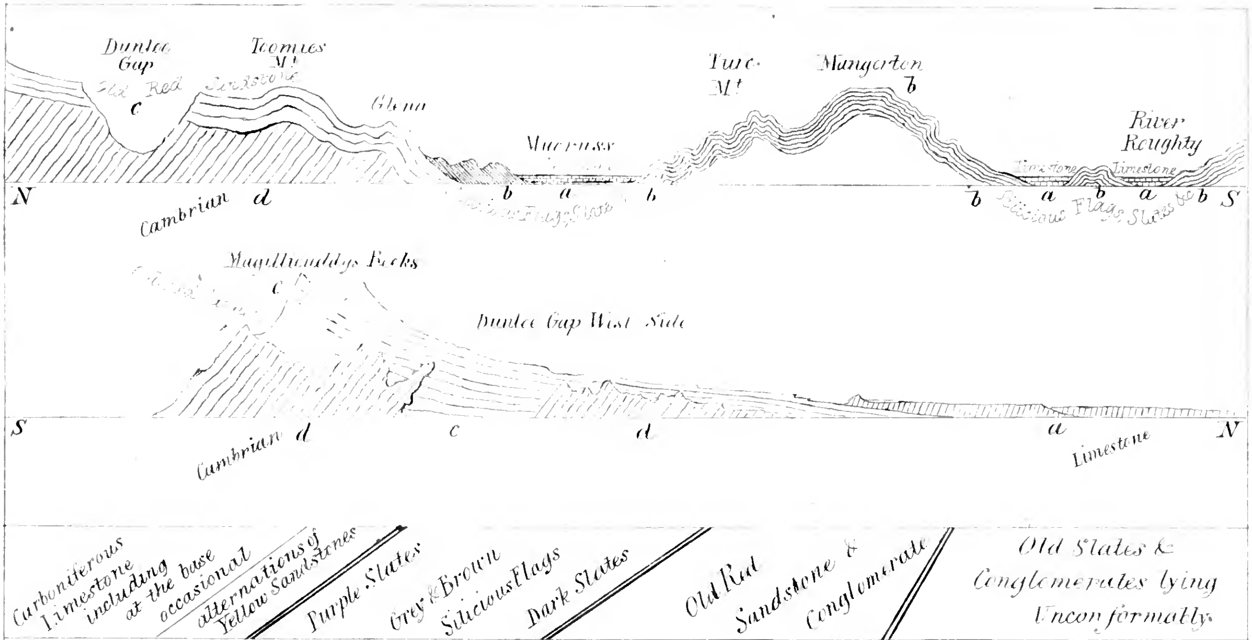
LXVI. *On the Succession of the Older Stratified Rocks in the Neighbourhood of Killarney and Dublin. By* Charles W. Hamilton, *Esq., F.G.S. & Sec. Geol. Soc. of Dublin.*

[With a Plate.]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE publication of Mr. Griffith's Map of Ireland has excited much attention, and obtained for the author the gratitude of geologists; it stands as a measure of what is supposed to be known of the structure of that country, and a guide to the most interesting subjects for future inquiry. You may therefore consider it as not wholly useless, if I point out very briefly how far the result of my own observations leads me to dissent from some of his conclusions; and even if future observers should convict me of incorrectness, I shall at least have pointed out an interesting field of inquiry, and helped to show that a large portion of Ireland is still a fit subject for geological debate. I shall confine my present letter to two



Order of Superposition in the South of Ireland.

J. Basire, lith.

districts, the immediate neighbourhood of Killarney, and the environs of Dublin, both of them attractive and easily accessible to English visitors.

Any geologist, in glancing over the southern portion of Mr. Griffith's map, would be struck by some novelties and many apparent incongruities in the arrangement of the strata. The greatest portion of the S.W. district is described as belonging to those older rocks which have been noticed by Mr. Weaver and others as underlying unconformably the old red sandstone of the Gaulty Mountains, &c.

In *direct junction* with these slaty rocks we see the coal-field at Calurbarna, the mountain limestone at Killarney, and the old red sandstone generally.

Under the title of old red sandstone we have included the coarse and uniform conglomerate of the Gaulty Mountains and the roofing slates of the Vale of Middleton; but while these are so thrown together, we perceive a regular and uniform band intervening between the old red sandstone and the mountain limestone, which Mr. Griffith has thought worthy of being separated into a distinct formation, and has designated the "yellow sandstone." This yellow sandstone appears as if invariably found immediately over the old red sandstone, and is surmounted in all places north of a line drawn from Dunmanway to Cove by the mountain limestone, but south of this line we have another new formation, which takes the place of the limestone, and which he designates as "carboniferous slate." The scale of rocks at the head of the map leads us to suppose either that these two formations alternate with each other, or that there is some doubt as to which is the oldest; into that doubt I can fully enter, for the only junctions I have seen are very obscure.

I need not allude to any details of the description given by Mr. Weaver of this district in the fifth volume of the Transactions of the Geological Society of London, as he has referred the whole of it to transition greywacké and limestone, an opinion from which Mr. Griffith and myself totally dissent; and it is with less hesitation that I do so, as the description of the gap of Dunloe shows the rapidity with which Mr. Weaver must have hurried over a place where he did not notice that junction between the Cambrian rocks and the old red sandstone, which exhibits, even from the high-road, a most striking instance of unconformability. (Pl. I. f. 1. and 2.) In Mr. Griffith's map, published by the Railway Commissioners in 1838, the country south of the line from Dunmanway to Cove was represented as belonging to the older rocks, and an unconformable junction was traced as occurring at Ring-

abella. Two years previously I had shown him my reasons for believing that the roofing slates there quarried were newer than the old red sandstone, and in the map published this year, he has adopted the correction. The section implied in the present map differs totally from that which I made in 1836, from Tralee to Kenmare (Journal of the Dublin Geol. Soc. vol. i. pt. 4.). He has represented the old red sandstone as merely a cap upon Magillicuddy's Reeks, (Pl. I. f. 2.) and assigned to the Cambrian epoch the mountains between them and Kenmare, which I represented as intermediate between the old red sandstone and mountain limestone.

Captain Portlock, in his two addresses from the chair of the Dublin Geological Society, has questioned even the existence of the old red sandstone upon Caranthuel.

In the "Outline of the Geology of Ireland," appended to the Second Report of the Railway Commissioners in 1838, Mr. Griffith describes the "mountain tract which comprises the whole of the county of Waterford, and large portions of the counties of Cork and Kerry, as containing 'two varieties of *transition slate*;' the second or newer slate usually rests unconformably on the older; the lower portion of its strata consists of alternating beds of brownish red quartzose conglomerate and coarse red slate; these strata are succeeded by alternations of red and grey quartz rock, red quartzose slate, and clay slate; the grains becoming finer as the beds accumulate, and recede further from the conglomerate, till at length the upper beds produce varieties of purple, brownish red and reddish grey clay slate, which are quarried for roofing slate, particularly in the valley of the Blackwater, as at Lismore." He then describes the *old red sandstone*, yellow sandstone, and mountain limestone, as *newer* and *superior* to these slates.

In the map published this year, he has included under the head of old red sandstone most of these slates, and cites as an example, the slates alluded to in the report as quarried near Lismore; but as in other places there appeared to be so wide a difference between his map and my sections, I again visited that part of the country this autumn, and although the want of a correct geographical map and the lateness of the season prevented me from working accurately in estimating thicknesses, &c., I saw enough to satisfy me that my own views in respect to the order of superposition had been correct.

As to classification, it appears to me that Mr. Griffith has thrown together two rocks which are very distinct and easy of recognition, namely;

The old red sandstone and its conglomerate, which varies but little in its lithological character, and is found in great thickness incumbent upon the edges of those Cambrian rocks which I compared and identified with a well-known type seen between Bangor and Ogwen in North Wales, and

Compact arenaceous rocks, varying in character from grey granular quartz to fine clay slate, of great thickness, occupying a wide extent of country, and in geological position agreeing with the upper part of the Devonian series described by Professor Sedgwick and Mr. Murchison as rising from beneath the culm or coal measures.

The scarcity of fossils and calcareous bands remains a difficulty in comparing the subdivisions with those of Devonshire, but even in the Wavellite of the Ringabella slates, and the fossil remains of Cove and Ballinhassig, we catch at points of resemblance which may serve as useful guides. These will be dwelt upon subsequently in detailed memoirs.

Between these latter and the mountain limestone there occur *occasionally* a few alternations of shales and sandstones of very variable thickness, but appearing to me never to exceed 100 feet, and *containing the fossils of the true mountain limestone, and none others*. These beds are what Mr. Griffith calls his "yellow sandstone" formation. Now I must differ from him, and think that they really mark nothing but the gradual transition from circumstances involving a purely arenaceous deposit, to those in which the calcareous matter originated; and that they are not more worthy of distinct notice in a geological map than would be any of those grits which form part of the carboniferous limestone formation of Yorkshire.

These sandy beds may be seen in their greatest development at Goat Island, near Youghal, as first described by Mr. Robert Ball, and are also visible on the coast of Dublin, at Portrane, Skerries, &c. I think I may mention the junction between the upper Devonian rocks and mountain limestone on the river Roughty, in the county of Cork, as one among many localities where they do not exist as laid down upon the map.

One section will explain the view I take of the whole southern series. (Pl. I. f. 1.)

In the gap of Dunloe, near Killarney, at the base of the Purple Mountain, the Cambrian conglomerates support unconformably a thickness of old red sandstone, which probably exceeds 500 feet. This occupies the summits of Tomies and Glena mountains, and at Glena dips rapidly to the south; here the transition is very sudden into a great thickness of black and brownish slates observable about Lady

Kenmare's cottage. As we cross the strike of the strata from thence towards Mucruss, we find exactly the same purple and greenish compact sandstones which occur in the neighbourhood of Cork, Middleton, &c. These, as we ascend in the series, pass into purple slates, and at the Old Weir Bridge the mountain limestone is supported by about 50 feet thick of alternating shales and sandstones, containing the fossils of the limestone, and identical with those of Goat Island, &c. If we follow the limestone to the southern extremity of Turc Lake, we find it again lying conformably upon the same series that I have described as occurring at Glena. I followed these along the course of the stream which runs into Turc Lake from the summit of Mangerton; and although the want of a good map and the extreme degree of contortion of the beds prevented me from accurately comparing them, I could not mistake the order of superposition, or doubt for a moment the identity of the strata upon the summit of Mangerton. The very upper-beds of the old red sandstone rise from underneath, but as we proceed to the south these are quickly again covered by newer rocks (Devonian), which pass through all the before-described changes, until in the river Roughty the carboniferous limestone lies on the purple slates without the intervention of any yellow sandstones.

I am therefore of opinion that the mountains lying between Caranthuel and Kenmare, and between Loch Gutane and the river Roughty, belong to a formation *intermediate between the old red sandstone and carboniferous limestone, and are constantly and clearly distinguishable from both;* and I have little doubt that whatever colour may be adopted to designate these rocks, will be found upon further research to take the place of far the greatest proportion of that which Mr. Griffith has laid down as the oldest slate (Cambrian).

As to the environs of Dublin, I have already, in the Journal of the Dublin Geological Society, expressed my belief, that the slates and siliceous flags which occur along the coast of Dublin and Louth, correspond with those of the vale of Middleton. I have since satisfied myself of the correctness of this view, and believe that all those tracts which between Dublin and Dundalk, along the course of the Boyne, and in the hills separating the counties of Cavan and Meath, have been described as the older greywacké by Mr. Griffith, Mr. Weaver, and others, are in reality all conformable and immediately inferior to the mountain limestone, and superior to the old red sandstone.

The coast of Dublin exhibits a well-developed series of calp, the lower limestone, with its inferior and sandy beds, as

well as of siliceous sandstones and slates, which I refer to an upper group of the Devonian rocks, and of these I have prepared sections and details for the Dublin Geological Society.

The lower figure in the accompanying plate will, it is hoped, sufficiently explain my views of the order of succession.

Dublin, October 23, 1839.

LXVII. *On the Connexion between the Boetian and the Middle-Arabic numerical Forms.* By J. O. Halliwell, *Esq., F.R.S., F.S.A., &c.**

In the *Comptes Rendus des Séances de l'Académie des Sciences* for the 7th of October, M. Libri and M. Chasles have commented at some length on certain portions of an essay on the Boetian numerical contractions which I ventured to place before the notice of the public some months ago. I hasten to explain some opinions there given, which I regret have been found to be expressed in too ambiguous terms to give sufficient satisfaction to the inquiries of those zealous and learned mathematicians.

With respect to the Bodleian manuscripts, I believe I have erred in asserting that in all three treatises *local position is clearly pointed out*, for in the tract of Berhelinus this is only to be inferred; but whatever doubt may arise in that case, I am fully persuaded that in the first Hattonian manuscript (No. 7), and in the treatise immediately preceding that of Berhelinus in the second Hattonian MS. (No. 112), not only is local position clearly pointed out, but there is quite as complete a system of Arithmetic as we find in the treatises of *Johannes de Sacro-Bosco*, or *Alexander de Villa Dei.* For, after all, the arithmetic of those authors is nothing more than the simplest adaptation of abacal principles, and every one knows that in contemporary MSS. of those treatises, and even in transcripts of as recent a period as the close of the 15th century, all the marginal operative examples are given in the abacal form, and only one step removed from the Arundel manuscript, where a blank space is used instead of the sipos. Again, even in the Arundel manuscript, which does not possess one higher principle than the passage in Boetius, although local position is not clearly pointed out, yet, as I have elsewhere stated, *the writer was evidently acquainted with the decuple value which a digit receives by its situation on the left of another*†.

* Communicated by the Author.

† Appendix to a Life of Sir Samuel Morland, p. 26.

The names alone are almost sufficient to show that the origin of these numerals is Eastern, if not from the Arabic; but it appears to me that sufficient attention has not been paid to the variations of the forms of the figures in different manuscripts. Now, in the tract *de ratione abaci*, we have invariably *two* lists of forms, and these again vary in MSS. of different ages. Let us make a brief comparison of a few of them.

1. In all the manuscripts to which I have referred, igin is the same, and agrees with the common middle-Arabic form.

2. Andras does not vary to any extent, but how easily is it convertible to the Arabic form!

3. Ornus in MS. Lansd. 842, is the same with ornus in MS. Arund. 343 *transversed*, and this is an example of a curious result of practice, the truth of which I hope to be able to establish universally. In MS. Harl. 3595, of the tenth century, we find two forms of ornus very nearly identical with the Arabic.

4. The form of arbas is not so readily convertible to the Arabic, but a bisection of this character in its second place in MS. Harl. 3595, and in MS. Lansd. 842, would make the loop of the middle-Arabic figure.

5. Quinas, as given in MS. Arund. 343, is *identical* with the middle-Arabic form. In MS. Harl. 3595, and in many other early manuscripts, this character is transversed.

6. Chalcus is evidently convertible to the common form by the mere obtainable effect of convenient expression.

7. Zenis, as given in the MS. Arundel. 343, *when transversed*, is identical with the middle-Arabic form. Examples of it, in that state, may be seen in MS. Lansd. 842, and MS. Harl. 3595.

8. Zemenias agrees in form with the common character.

9. Celentis agrees, by transversion, with the common form. The second form of this figure in MS. Lansd. 842 is transversed.

I am almost inclined to believe from these examples, and from others equally conclusive which can be brought forward, that, in the case of middle numerals, transversion is invariably true in the change of form they underwent, provided that any reasonable theory is thereby supported. Perhaps also we may arrive at an additional argument from the Eastern manner of writing from the right to the left; for what is more probable than, supposing the case of a Latin scribe copying the numerals from the Arabic, that he should turn them *upside down*, for the facility thus afforded by that means of copying them in the proper order? Thus would the transversion be satisfactorily accounted for, and without a conjecture so vio-

lently improbable as to induce one to reject it on the grounds of no immediate evidence*.

Boetius immediately precedes the passage *de ratione abaci* by definitions of numbers, *digiti*, *articuli*, and *compositi*,—a division which, I may observe, could only have been subservient to the use of the Abacus, and the very same which Alexander de Villa Dei and others, expressly using the *Arabic* calculus, have employed in their works†. This is a strong argument; and in MS. Trin. Coll. Cantab. R. xv. 16, we find the Boetian digital forms identical with the middle-Arabic with the exception of a very slight deviation in ornus, and having arbas written on one side.

I venture to exhibit the following conclusions, which I hope will not be considered too premature.

1. That the Boetian contractions are wholly independent of the middle-Arabic in their introduction into Europe.

2. That the Boetian contractions formed a distinct system of decimal numeration, borrowed from the East, and introduced through the Latins into Eastern Europe.

3. That the Boetian notation was anterior to the introduction of the middle-Arabic numerals through Spain.

Professor Davies, in a key to the new edition of Hutton's Course of Mathematics, now on the eve of publication, has given some entirely new views relative to the period of change between the abacal and concentrated modes of operation, and it is on that account that I defer entering into that part of the subject, because his arguments are so forcible, so conclusive, and agree so well with an examination of early manuscripts, that an abridgement would sensibly deteriorate their value; moreover, as the work itself will ere long be so accessible to every one, there can be no necessity.

I hope shortly to be able to put in a form fit for publication some researches on the Leopoldine Numerical Contractions, which form a system of numeration that has hitherto entirely escaped the researches of every writer on the history of arithmetic. Thus within the space of a few months will a completely new face be laid on the history of the Hindoo arithmetic in Europe during the middle ages.

* I entered into a full explanation of the results of this conjecture in a paper read before the Royal Society of Literature, but which is not yet printed.

† Rara Mathematica, p. 2, 74; Professor Peacock's History of Arithmetic, in *Encyc. Metrop.*

LXVIII. *Researches in the Undulatory Theory of Light continued: On the Absorption of Light.* By JOHN TOVEY, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AT the conclusion of my last paper (L. & E. Phil. Mag. vol. xiv. p. 323.) I said that I expected to be able to show that our formulæ were adequate to explain the cause of the absorption of light. This expectation was founded on the circumstance that the quantity which we have denoted by k, has, without any reason, been tacitly assumed to be entirely real; while it is obvious that, if k be partly imaginary, the formulæ require to be transformed into others which will indicate an absorption depending on k and x. The values of k depend on the nature of the medium; and it is well known that every medium absorbs more or less of the light which falls on it. Hence we infer that these values cannot be entirely real. Indeed, if they were so, the formulæ which we have previously deduced would show that no absorption could take place.

To effect the required transformation of our formulæ, I have had recourse to a method of investigation more general, and, I think, more easy, than any which we have previously used. This method I now proceed to develop; setting out from the differential equations, given at page 11. vol. xii. of your Journal, viz.

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= m\,\Sigma\left\{\phi(r)\,\Delta\eta + \psi(r)\left(\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta\right)\Delta y\right\}, \\ \frac{d^2\zeta}{dt^2} &= m\,\Sigma\left\{\phi(r)\,\Delta\zeta + \psi(r)\left(\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta\right)\Delta z\right\}. \end{aligned}\right\} \quad (1.)$$

In deducing these equations the masses of the molecules were supposed to be all equal*. When this is not the case, m, which denotes any one of these masses, must be placed under the sign, Σ, of summation. If for the sake of abridgement, we put

$$\left.\begin{aligned} m\,\{\phi(r) + \psi(r)\,\Delta y^2\} &= p, \\ m\,\{\phi(r) + \psi(r)\,\Delta z^2\} &= p', \\ m\,\psi(r)\,\Delta y\,\Delta z &= q, \end{aligned}\right\} \quad (2.)$$

the equations (1.) will become

$$\left.\begin{aligned} \frac{d^2\eta}{dt^2} &= \Sigma\left(p\,\Delta\eta + q\,\Delta\zeta\right), \\ \frac{d^2\zeta}{dt^2} &= \Sigma\left(p'\,\Delta\zeta + q\,\Delta\eta\right); \end{aligned}\right\} \quad (3.)$$

which are true whether the masses are equal or unequal.

* See L. & E. Phil. Mag. vol. viii. p. 7—9.

Since we suppose x to be normal to the wave surface, and, consequently, the general expressions for the displacements η and ζ to be functions of x and t, we may conceive each of these expressions to be developed in a series of the powers and products of e^x and e^t: where e is the number whose hyperbolical logarithm is unity. Every term of the developments will be of the form $\alpha\, e^{\nu t + \kappa x}$: where α, ν, κ, are constant quantities.

Suppose $\quad \eta = \alpha\, e^{\nu t + \kappa x}, \; \zeta = \rho\, \alpha\, e^{\nu t + \kappa x}, \qquad (4.)$

and put $\quad e^{\nu t + \kappa x} = \omega: \qquad (5.)$

we shall then find

$$\frac{d^2 \eta}{d t^2} = \nu^2 \alpha\, \omega, \qquad \frac{d^2 \zeta}{d t^2} = \nu^2 \rho\, \alpha\, \omega, \qquad (6.)$$

$$\Delta \eta = (e^{\kappa \Delta x} - 1)\, \alpha\, \omega, \qquad \Delta \zeta = (e^{\kappa \Delta x} - 1)\, \rho\, \alpha\, \omega. \qquad (7.)$$

Put

$$\begin{aligned} \Sigma p\, (e^{\kappa \Delta x} - 1) &= s, \\ \Sigma p'\, (e^{\kappa \Delta x} - 1) &= s', \\ \Sigma q\, (e^{\kappa \Delta x} - 1) &= s_{,}, \end{aligned} \qquad (8.)$$

and substitute the values (6.) and (7.) in (3.); we shall then have, on omitting the common factor $\alpha\, \omega$,

$$\begin{aligned} \nu^2 &= s + \rho\, s_{,}, \\ \nu^2 &= s' + \frac{s_{,}}{\rho}. \end{aligned} \qquad (9.)$$

Hence it appears that, when the arbitrary quantities ν, ρ, κ, are made to satisfy these two conditions, the expressions (4.) are a particular solution of the equations (3.). Now, since these equations are of the first degree, they may be satisfied, not only by the expressions (4.), but by the sums of any number of expressions of the same form; therefore we may put

$$\eta = \Sigma\, \alpha\, e^{\nu t + \kappa x}, \qquad \zeta = \Sigma\, \rho\, \alpha\, e^{\nu t + \kappa x}; \qquad (10.)$$

which will represent the general values of η and ζ, developed, as we have previously supposed, in series of the powers and products of e^t and e^x.

It is obvious, by (10.), that the values of ν must be entirely imaginary; for, if not, the displacements, η and ζ, would contain terms increasing indefinitely with the time; which they cannot do, because the motions under consideration are, by hypothesis, merely vibratory. We may therefore put

$$\nu = n \sqrt{-1}, \qquad (11.)$$

and change, accordingly, the equations (9.) into

$$\left.\begin{aligned} n^2 + s + \rho\, s_{\prime} &= 0\,, \\ n^2 + s' + \frac{s_{\prime}}{\rho} &= 0\,. \end{aligned}\right\} \qquad (12.)$$

By eliminating ρ from (12.) we have

$$(n^2 + s)\ (n^2 + s') = s_{\prime}^2\,. \qquad (13.)$$

The sums s, s', $s_{\prime}$, comprise only $\varkappa$ and quantities depending on the nature of the medium; therefore, the last equation determines the value of $\varkappa$, supposing that of n to be given.

Whatever be the nature of $\varkappa$, we may, by the principles of analysis, suppose $\quad \varkappa = h\,(\cos i + \sqrt{-1}\,\sin i)$: $\qquad$ (14.)
where h and i are real quantities. Therefore, since

$$e^{\varkappa\,\Delta x} = 1 + \frac{\varkappa\,\Delta x}{1} + \frac{\varkappa^2\,\Delta x^2}{1\cdot 2} + \frac{\varkappa^3\,\Delta x^3}{1.\,2.\,3.} + \&c. \qquad (15.)$$

and

$$(\cos i + \sqrt{-1}\,.\,\sin i)^m = \cos m\,i = \sqrt{-1}\,.\,\sin m\,i\,, \qquad (16.)$$

(where m is any number,) we have

$$\left.\begin{aligned} e^{\varkappa\,\Delta x} - 1 = \frac{h\cos i}{1}\,\Delta x + \frac{h^2\cos 2\,i}{1\,.\,2}\,\Delta x^2 + \&c. \\ + \sqrt{-1}\,.\left(\frac{h\sin i}{1}\,\Delta x + \frac{h^2\sin 2\,i}{1\,.\,2}\,\Delta x^2 + \&c.\right). \end{aligned}\right\} \qquad (17.)$$

By substituting this development of $(e^{\varkappa\,\Delta x} - 1)$ in the equations (8.), and, for the sake of abridgement, putting

$$\left.\begin{aligned} &\mathrm{A} = \Sigma\,p\,\Delta x\,, && \mathrm{A}' = \Sigma\,p'\,\Delta x\,, \\ &\mathrm{A}_{\prime} = \frac{\Sigma\,p\,\Delta x^2}{2}\,, && \mathrm{A}'_{\prime} = \frac{\Sigma\,p'\,\Delta x^2}{2}\,, \\ &\mathrm{A}_2 = \frac{\Sigma\,p\,\Delta x^3}{2\,.\,3}\,, && \mathrm{A}'_2 = \frac{\Sigma\,p'\,\Delta x^3}{2\,.\,3}\,, \\ &\&c. && \&c. \\ &\mathrm{B} = \Sigma\,q\,\Delta x\,, \\ &\mathrm{B}_1 = \frac{\Sigma\,q\,\Delta x^2}{2}\,, \\ &\mathrm{B}_2 = \frac{\Sigma\,q\,\Delta x^3}{2\,.\,3}\,, \\ &\&c. \end{aligned}\right\} \qquad (18.)$$

and

$$\left.\begin{aligned} \sigma &= \mathrm{A}\,h\cos i + \mathrm{A}_{\prime}\,h^2\cos 2\,i + \mathrm{A}_2\,h^3\cos 3\,i + \&c. \\ \sigma' &= \mathrm{A}'\,h\cos i + \mathrm{A}'_{\prime}\,h^2\cos 2\,i + \mathrm{A}'_2\,h^3\cos 3\,i + \&c. \\ \sigma_{\prime} &= \mathrm{A}\,h\sin i + \mathrm{A}_{\prime}\,h^2\sin 2\,i + \mathrm{A}_2\,h^3\sin 3\,i + \&c. \\ \sigma'_{\prime} &= \mathrm{A}'\,h\sin i + \mathrm{A}'_{\prime}\,h^2\sin 2\,i + \mathrm{A}'_2\,h^3\sin 3\,i + \&c. \end{aligned}\right\} \qquad (19.)$$

$$\sigma_2 = \mathrm{B}\, h \cos i + \mathrm{B}_{,} h^2 \cos 2\, i + \mathrm{B}_2\, h^3 \cos 3\, i + \&c.$$
$$\sigma_3 = \mathrm{B}\, h \sin i + \mathrm{B}_{,} h^2 \sin 2\, i + \mathrm{B}_2\, h^3 \sin 3\, i + \&c.$$

we find

$$s = \sigma + \sqrt{-1} \,.\, \sigma_{,}\,, \qquad s' = \sigma' + \sqrt{-1} \,.\, \sigma_{,}'\,, \qquad s_{,} = \sigma_2 + \sqrt{-1} \,.\, \sigma_3 \,. \tag{20.}$$

Now suppose

$$\rho = \beta\,(\cos\gamma + \sqrt{-1} \,.\, \sin\gamma)\,, \tag{21.}$$

and substitute the values of s, s', $s_{,}$, and ρ, in (12.); then, since n is entirely real, we shall have

$$\begin{aligned} n^2 + \sigma + \beta\,(\sigma_2 \cos\gamma - \sigma_3 \sin\gamma) &= 0\,, \\ n^2 + \sigma' + \frac{1}{\beta}(\sigma_2 \cos\gamma + \sigma_3 \sin\gamma) &= 0\,, \\ \sigma_{,} + \beta\,(\sigma_2 \sin\gamma + \sigma_3 \cos\gamma) &= 0\,, \\ \sigma'_{,} - \frac{1}{\beta}\,(\sigma_2 \sin\gamma - \sigma_3 \cos\gamma) &= 0\,. \end{aligned} \tag{22.}$$

The four equations (22.) determine the values of h, i, β, γ, corresponding to any given value of n; and it may easily be seen that these equations will not be affected if we change the sign of n, or if we change, simultaneously, the signs of i and γ. Therefore, since the general values of η and ζ are the sums of all their particular values, we may give to ν, $\varkappa$, ρ, in (10.), not only the values (11.), (14.), (21.), but also those which result from them, by changing the signs of n, i, γ. If we do so, and, for the sake of abridgement, put

$$h \cos i = \epsilon\,, \qquad h \sin i = k\,, \tag{23.}$$

$$\begin{aligned} \beta\,(\cos\gamma + \sqrt{-1} \,.\, \sin\gamma) &= \rho_1\,, \\ \beta\,(\cos\gamma - \sqrt{-1} \,.\, \sin\gamma) &= \rho_2\,, \end{aligned} \tag{24.}$$

we shall have

$$\begin{aligned} \eta &= \Sigma\, \{e^{\epsilon x}\,(\alpha_1 e^{(nt+kx)\sqrt{-1}} + \alpha_2\, e^{-(nt+kx)\sqrt{-1}})\}\,, \\ \zeta &= \Sigma\, \{e^{\epsilon x}(\rho_{,}\, \alpha_{,}\, e^{(nt+kx)\sqrt{-1}} + \rho_2\, \alpha_2\, e^{-(nt+kx)\sqrt{-1}})\}; \end{aligned} \tag{25.}$$

where $\alpha_{,}$, α_2, are values of α, and are entirely arbitrary.

By changing the last expressions into circular functions, we find

$$\begin{aligned} \eta &= \Sigma\, \{a\, e^{\epsilon x} \sin\,(n\,t + k\,x + b)\}\,, \\ \zeta &= \Sigma\, \{a'\, e^{\epsilon x} \sin\,(n\,t + k\,x + b')\}\,: \end{aligned} \tag{26.}$$

where a, b, a', b', are determined by the equations

$$\begin{aligned} a \sin b &= \alpha_{,} + \alpha_2\,, & a \cos b &= (\alpha_{,} - \alpha_2)\sqrt{-1}\,, \\ a' \sin b' &= \rho_{,}\, \alpha_{,} + \rho_2\, \alpha_2\,, & a' \cos b' &= (\rho_{,}\, \alpha_{,} - \rho_2\, \alpha_2)\sqrt{-1}\,. \end{aligned} \tag{27.}$$

Since η and ζ are entirely real, it appears, by (26.) and (27,), that $\alpha_{\prime}$ and α_2 must be of the forms

$$\alpha_{\prime} = c_{\prime} + c_2 \sqrt{-1}\,, \qquad \alpha_2 = c_{\prime} - c_2 \sqrt{-1}\,: \qquad (28.)$$

therefore, by (27.)

$$a \sin b = 2 c_{\prime}\,, \qquad a \cos b = -2 c_2\,. \qquad (29.)$$

Again, by (24.) and (27.),

$$\left.\begin{aligned} a' \sin b' &= \beta (\cos \gamma + \sqrt{-1}\,.\sin \gamma)\, \alpha_{\prime} \\ &\quad + \beta (\cos \gamma - \sqrt{-1}\,.\sin \gamma)\, \alpha_2\,, \\ a' \cos b' &= \beta (\cos \gamma + \sqrt{-1}\,.\sin \gamma)\, \alpha_{\prime} \sqrt{-1} \\ &\quad + \beta (\cos \gamma - \sqrt{-1}\,.\sin \gamma)\, \alpha_2 \sqrt{-1}\,; \end{aligned}\right\} \qquad (30.)$$

hence, by (28.),

$$\left.\begin{aligned} a' \sin b' &= 2 \beta\, (c_{\prime} \cos \gamma - c_2 \sin \gamma)\,, \\ a' \cos b' &= -2 \beta\, (c_2 \cos \gamma + c_{\prime} \sin \gamma)\,; \end{aligned}\right\} \qquad (31.)$$

and hence, by (29.)

$$\left.\begin{aligned} a' \sin b' &= a \beta \sin (b + \gamma)\,, \\ a' \cos b' &= a \beta \cos (b + \gamma)\,; \end{aligned}\right\} \qquad (32.)$$

therefore $\quad a' = a \beta\,, \qquad b = b + \gamma\,, \qquad (33.)$

or $\quad a' = - a \beta\,, \qquad b = b + \gamma + \pi\,. \qquad (34.)$

Since $\alpha_{\prime}$ and α_2 are arbitrary so also are $c_{\prime}$ and c_2, by (28.); and consequently a and b, by (25.).

By (23.) and (26.) we have

$$\left.\begin{aligned} \eta &= \Sigma \{a\, e^{\epsilon x} \sin (n t + k x + b)\}\,, \\ \zeta &= \Sigma \{\beta\, a\, e^{\epsilon x} \sin (n t + k x + b + \gamma)\}\,. \end{aligned}\right\} \qquad (35.)$$

In these expressions, which are equally general with the formulæ (10.), all the symbols denote real quantities.

We have remarked that the equations (22.) determine the values of h, i, β, γ, for any given value of n. The values of h, i, determine those of ϵ, k, by (23.). Now the intensity of the light depends on $a\, e^{\epsilon x}$ and $\beta\, a\, e^{\epsilon x}$, so that if the vibrations in the ray under consideration be represented by (35.), its intensity will be expressed by

$$\Sigma \{(a\, e^{\epsilon x})^2 + (\beta\, a\, e^{\epsilon x})^2\}\,.$$

(See Airy's Tract on Light, art. 23, and note.) Hence if we suppose the origin of x to be at the surface of any medium on which a ray of light falls, we perceive that, unless ϵ be zero, the intensity of the light will vary with x; and since experience shows, that whatever be the medium which a ray of light traverses, its intensity is thereby diminished, we infer that ϵ must have sensible values, and that these values must be negative.

Now, by (23.), ε is connected with k; hence it follows that the diminution of the intensity of the light, in traversing any medium, depends on the thickness of the medium and the length of the wave.

This inference agrees with what we learn by experiment; therefore we may say that our formulæ afford, so far, an explanation of the cause of the absorption of light. The cause subsists in the values of $\varkappa$ given by the equation (13.). In all media for which these values are, severally, partly real and partly imaginary, absorption must take place.

It is my intention to pursue this subject further in another paper.

I am, Gentlemen, yours, &c.,
JOHN TOVEY.

Littlemoor, near Clitheroe, Nov. 4, 1839.

LXIX. *Observations on* Mr. Lyell's *Paper, entitled,* "*On the tubular Cavities filled with Gravel and Sand, called Sand-pipes, in the Chalk near Norwich.*" *By* WILLIAM STARK, *F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

DEAR SIRS,

YOUR Magazine of the present month contains a paper by Mr. Lyell on the "Sand-pipes in the chalk near Norwich," upon which I beg to offer a few remarks.

In the first place allow me to observe that the description which Mr. Lyell has given of the forms of the chalk pits and sand-pipes in this neighbourhood is very accurate, and the engravings by which it is illustrated are excellent representations of the originals. The hypothesis, however, that he advances relative to the origin of their formation, is, in my opinion, far from being satisfactory. The order of arrangement which he has adopted does not appear to me the most suitable: nevertheless I will take it as it stands.

He says, "For the distance of several inches, or even in some places four or five feet from its junction with the sand-pipes, the chalk at Eaton is moist and softened, and becomes friable when dried, and is discoloured by containing a slight mixture of fine sand, clay and iron, the same chalk being *quite pure*, and perfectly soluble in acids at points more remote from the pipes."

This does not correspond with the result of my experiments upon *the same chalk*, however remote from the sand-pipes. I did not find it "quite pure"; *it was not* "*perfectly*

soluble in acids" ; for both with hydro-chloric and acetic acids it left a residuum which proved to be silica and alumina. I notice this fact as being of importance to a theory I am about to advance relative to the chalk formation.

As to the origin of the sand-pipes, Mr. Lyell observes: "We have now to consider in what manner these cylindrical hollows have been first formed and then filled with gravel and sand. If no pipes but those of the smallest size had occurred, we might have imagined that the top roots of large trees had first pierced the chalk, and then after growing to their full size and decaying had left a vacant space into which loam and gravel fell. But when we reflect on the dimensions of some of the pipes, we at once perceive that more powerful causes must be appealed to."

"On consideration of all the facts we can scarcely hesitate to admit the following conclusions:—That the chalk has been removed by the corroding action of water charged with acid, in which the siliceous nodules being insoluble were left *in situ* in the smaller pipes after the calcareous matrix had been dissolved."

It appears to me that all this may be satisfactorily accounted for without presuming that the "water was charged with acid." The appearance of every mass of chalk which I have seen warrants our concluding that it was deposited at different times from a solution or suspension in water. The layers which a section of any mass of chalk exhibits on excavation, proves that a deposit of a limited depth has taken place and become *partially solidified* before a super deposit had commenced, and consequently there must have been a very considerable lapse of time between the lowermost and the uppermost stratum of chalk in any deep mass of that substance. We cannot hesitate to admit this, not only from the evident lines (generally horizontal, but sometimes with a slight dip) which every perpendicular face of chalk exhibits, but also from the fact, that upon each of those lines, or rather tables, are deposited dark-coloured particles, principally composed of siliceous and argillaceous matter, as if they were precipitated from an upper stratum of the calcareous mass, whilst it was in a semi-fluid state, and after the under, or previously deposited, stratum had been desiccated. Every layer of chalk appears to me to have been thus deposited, till the aggregate formed the immense calcareous masses we now witness.

Admitting this hypothesis, (and I think the facts of the case warrant such an admission) there must have occurred in the course of solidification considerable fissures in these deposits;

for every chemist knows, that during the process of drying precipitates of this nature, there are always fissures produced in the precipitated substance as the fluid evaporates or filters away. Such effects must have been a necessary consequence in the chalk as it approximated to dryness, and hence the primary cause of the cavities we are investigating. The difference of size in the cavities is, I conceive, a consequence that would inevitably result, not only from some parts of a mass being more exposed to the sun than others, but also to various circumstances which would cause partial drying of the chalk; and above all to the failure of support from the under strata, from subterraneous springs, &c. Indeed there seems reason to believe that the largest sand-pipes have been formed by the complete division of a mass of chalk from the bottom to the top arising from the above-named causes.

Mr. Lyell further observes that "Large unrounded nodules of flints, found in the sand-pipes, still preserve the original form and white coating." And in the smaller pipes "horizontal layers of siliceous nodules still remain *in situ*, not having been removed, together with the chalk in which they must have been originally imbedded."

If these cavities were formed by the action of "water charged with acid," as Mr. Lyell supposes, how is it that it did not dissolve the chalk from the surface of those flints "*in situ*," upon which its action must have been as great as upon any other part of the surface? They remain, however, still with the chalk adhering to them, as pure and as perfect as that in any other part of the mass.

Mr. Lyell goes on to state, "It is clear from the manner in which the large detached flints are dispersed through the contents of the wide sand-pipes, that excavation and filling of the pipes were gradual and contemporaneous processes. For had the tubes, some of which are from 50 to 60 feet deep, and seven yards or more wide, been hollowed out of the chalk before the introduction of any foreign matter from above, great heaps of unrounded flints must have fallen to the bottom, derived from all those intersected layers of flint which formed part of the chalk above."

There is no doubt but they have done so, and I do not see that it follows, on that account, that excavation and filling up were contemporaneous processes; for we have no data to suppose that what is *now* the bottom of a wide and deep sand-pipe was so *originally*: such an immense division in masses of chalk must have been partially filled up by the sides of the hollows being washed down to the bottom of the cavity, and most probably have covered over the flints which fell down it

hundreds of feet. For as the age of the chalk is undoubtedly great, indeed beyond that of the superimposed strata, there must have been a continued wearing away of the sides of the cavities by the action of water, and a consequent transit of the *upper* to the *lower* parts of the hollow, ages before the clay, sand, and gravel were produced.

The most remarkable part of Mr. Lyell's paper is the following: he says, "It only remains for us to inquire how waters charged with acid may most naturally be conceived to have produced such hollows. If some of the largest pipes of which the bottom has not yet been reached be prolonged indefinitely downwards and connected with deep fissures, we may suppose that springs charged with carbonic acid rose up at some former period through the chalk and crag while they were still submerged, as we know that in many parts of the bed of the sea such springs do break forth. In proportion as the chalk was corroded, the incumbent substances would subside into the hollow thus formed and the water would freely percolate the matter thus intruding itself, dissolving any calcareous ingredients which may be associated with it, and still continue widening the tube by corroding its walls."

Had this been the case, we may presume that the form of the sand-pipes would have been exactly the reverse of what they now are; they would have assumed the form of *cones*, but not *inverted cones*; for the action of such waters must have been greatest at the bottom where they first entered the chalk, not only from the effect of mechanical force, but chemical action; and the destruction must have been greatest, and the widening consequently largest, at the *under* part of such calcareous masses.

Mr. Lyell seems rather to doubt this part of his hypothesis himself, for he says it will not account for the form of the greater number of sand-pipes, as they "have been found to diminish gradually downwards to a point."

This is precisely the form they would assume, supposing that the fissures were produced, as I have stated; the continued washing down of the upper parts of the chalk from heavy rains, and streams of water of any kind, must have the tendency to widen the upper part of each cavity, and progressively to fill up the lower part of it, by carrying down the walls gradually from top to bottom, leaving portions of the chalk in the transit of the fluid, and forming the smooth-sided funnel-like shape that we now observe in the majority of sand-pipes.

The filling up of these pipes was undoubtedly an *after work*, probably ages after; and the same cause that produced

the *shape* of the sand-pipes in the chalk would operate to fill them with the superincumbent strata. We find the order in which the materials lie in the pipes corresponding precisely with that of the superimposed strata on the surface of the chalk, the clayey and ochry matter first; the sand and gravel next; then the water-worn pebbles; and, lastly, in many cases, boulders. The whole appears arranged like the table of contents in a book, and tells the tale as well.

I remain, dear Sirs, yours truly,

Norwich, Oct. 15, 1839. WM. STARK.

LXX. *On the Appearance of a Luminous Point seen through a telescope, the object-glass of which has an aperture of the form of a scalene triangle.* By W. H. MILLER, *M.A., F.R.S., F.G.S., Fellow and Tutor of St. John's College, and Professor of Mineralogy in the University of Cambridge.*

WHEN a point of light is seen through a telescope having a small triangular aperture, the telescope having been adjusted so as to show the point distinctly with its undiminished circular aperture, there will be seen, in place of a well-defined image of the luminous point, a bright disc, from which proceed, in directions perpendicular to the sides of the aperture, six rays, accompanied on each side, when the light is sufficiently strong, by parallel rows of bright points, decreasing in intensity as they recede from the principal rays. The intensity of the light at any point in the field of view may be expressed by a formula of great simplicity, considering the complicated nature of the phænomenon it represents, which first appeared in Professor Schwerd's elaborate treatise on diffraction (Die Beugungserscheinungen aus den Fundamentalgesetzen der Undulationstheorie analytisch entwickelt und in Bildern dargestellt. Mannheim, 1835.). This formula may be deduced in the following manner.

Let K be the centre of the object-glass; O the geometric focus to which the rays proceeding from the luminous point converge, after refraction, through the object-glass; M any point in a plane through O perpendicular to O K; A B C the triangular aperture. Draw M H parallel to O K meeting the plane of A B C in H, and D A E, B F, C G, Q P R perpendicular to H K. Let F G, G E, E F, the orthogonal projections of B C, C A, A B upon H K, be equal to α, β, γ respectively; S the area of A B C, O K $= b$, K H $= p$, K R $= x$, A D $= e$. Then (Airy's Tracts, Undulatory Theory of Optics, art. 80.) the wave transmitted through an aperture A P Q D would cause at M a displacement.

$$=\int_x \int_y \sin\left\{\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{p}{b}\,x\right)\right\},$$

between limits corresponding to the boundary of A P Q D,

$$=\int_x \mathrm{P\,Q}\,\sin\left\{\frac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \frac{p}{b}\,x\right)\right\},$$

from $x = \mathrm{K\,E}$ to $x = \mathrm{K\,R}$.

$\mathrm{PQ}=e\,\dfrac{\mathrm{K\,F}-x}{\gamma}$. Let $\dfrac{\pi\,p}{b\,\lambda} = m$, $\dfrac{2\,\pi}{\lambda}\left(v\,t - \mathrm{B} + \dfrac{p}{b}\,\mathrm{K\,E}\right) = \phi$.

Then the wave transmitted through an aperture A B D would cause at M a displacement

$$=\int_x \frac{e}{\gamma}\,(\mathrm{K\,F} - x)\sin\{\phi + 2\,m\,(x - \mathrm{K\,E})\},$$

from $x = \mathrm{K\,E}$ to $x = \mathrm{K\,F}$,

$$= e\,\frac{1-\cos 2\,m\,\gamma}{4\,m^2\,\gamma}\sin\phi + e\left(\frac{1}{2\,m} - \frac{\sin 2\,m\,\gamma}{4\,m^2\,\gamma}\right)\cos\phi.$$

In like manner the wave transmitted through an aperture A C D would cause at M a displacement

$$= e\,\frac{1-\cos 2\,m\,\beta}{4\,m^2\,\beta}\sin\phi + e\left(\frac{1}{2\,m} - \frac{\sin 2\,m\,\beta}{4\,m^2\,\beta}\right)\cos\phi.$$

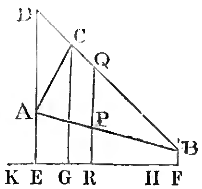
Hence, observing that $2\,\mathrm{S} = e\,\alpha$, the wave transmitted through the aperture A B C will produce at M a displacement

$$= \frac{\mathrm{S}}{2\,m^2\,\alpha\,\beta\,\gamma}\left\{\beta\,(1 - \cos 2\,m\,\gamma) - \gamma\,(1 - \cos 2\,m\,\beta)\right\}\sin\phi$$

$$-\frac{\mathrm{S}}{2\,m^2\,\alpha\,\beta\,\gamma}\left\{\beta\sin 2\,m\,\gamma - \gamma\sin 2\,m\,\beta\right\}\cos\phi.$$

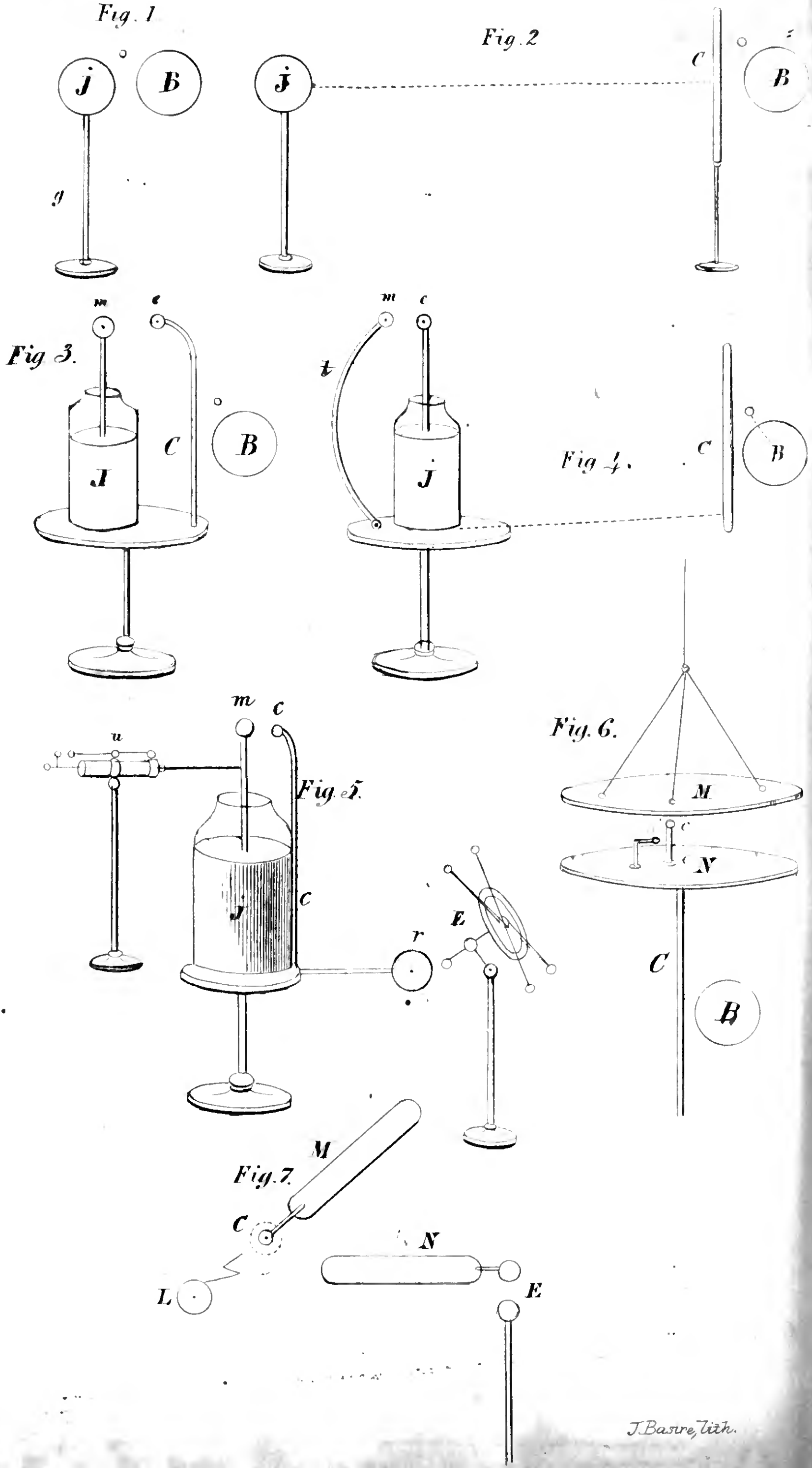
The intensity of the light at M is expressed by the sum of the squares of the coefficients of $\sin\phi$ and $\cos\phi$.

$$\{\beta\,(1-\cos 2\,m\,\gamma) - \gamma\,(1-\cos 2\,m\,\beta)\}^2$$
$$+\ \{\beta\sin 2\,m\,\gamma - \gamma\sin 2\,m\,\beta\}^2$$
$$= 4\,\{\beta\,(\sin m\,\gamma)^2 - \gamma\,(\sin m\,\beta)^2\}^2$$
$$+\ 4\,\{\beta\sin m\,\gamma\cos m\,\gamma - \gamma\sin m\,\beta\cos m\,\beta\}^2$$
$$= 4\,\beta^2\,(\sin m\,\gamma)^2 + 4\,\gamma^2\,(\sin m\,\beta)^2$$
$$-\ 8\,\beta\,\gamma\sin m\,\beta\sin m\,\gamma\cos m\,\alpha,$$



since $\gamma = \alpha + \beta$.

In the above expression any two of the quantities α, β, γ, may be interchanged without altering its value. Hence, the intensity of the light at M will be expressed by



J. Basire, lith.

$$\frac{S^2}{(m\,\alpha)^2}\left\{\left(\frac{\sin m\,\beta}{m\,\beta}\right)^2+\left(\frac{\sin m\,\gamma}{m\,\gamma}\right)^2-2\frac{\sin m\beta}{m\,\beta}\frac{\sin m\gamma}{m\,\gamma}\cos m\,\alpha\right\},$$

or by

$$\frac{S^2}{(m\,\beta)^2}\left\{\left(\frac{\sin m\gamma}{m\,\gamma}\right)^2+\left(\frac{\sin m\,\alpha}{m\,\alpha}\right)^2-2\,\frac{\sin m\gamma}{m\,\gamma}\frac{\sin m\alpha}{m\,\alpha}\cos m\,\beta\right\},$$

or by

$$\frac{S^2}{(m\gamma)^2}\left\{\left(\frac{\sin m\alpha}{m\,\alpha}\right)^2+\left(\frac{\sin m\beta}{m\,\beta}\right)^2-2\,\frac{\sin m\alpha}{m\,\alpha}\frac{\sin m\beta}{m\,\beta}\cos m\,\gamma\right\}.$$

The sum of the squares of the coefficients of sin ϕ and cos ϕ may be put in another form.

$$\{\beta\,(1-\cos 2\,m\,\gamma)-\gamma\,(1-\cos 2\,m\,\beta)\}^2$$
$$+\ \{\beta\sin 2\,m\,\gamma-\gamma\sin 2\,m\,\beta\}^2$$
$$=2\beta^2+2\,\gamma^2-2\beta\gamma-2\beta\,\gamma\cos 2m(\gamma-\beta)+2(\beta\gamma-\gamma^2)\cos 2\,m\beta$$
$$+\,2\,(\beta\,\gamma-\beta^2)\cos 2\,m\,\gamma$$
$$=4\,\beta\gamma\,(\sin m\,\alpha)^2+4\,\gamma\,\alpha\,(\sin m\,\beta)^2$$
$$+\,4\,\alpha\,\beta\,\,(\sin m\,\gamma)^2.$$

Hence the intensity of the light at M

$$=\frac{S^2}{m\alpha\,.\,m\beta\,.\,m\gamma}\left\{\frac{(\sin m\,\alpha)^2}{m\,\alpha}+\frac{(\sin m\,\beta)^2}{m\,\beta}-\frac{(\sin m\,\gamma)^2}{m\,\gamma}\right\},$$

γ being that projection which is equal to the sum of the other two.

St. John's College, Nov. 2, 1839.

LXXI. *On Lightning Conductors, and on certain Principles in Electrical Science; being an investigation of Mr. Sturgeon's Experimental and Theoretical Researches in Electricity, published by him in the "Annals of Electricity," &c. By* W. Snow Harris, *Esq., F.R.S.*

(Illustrated by Plate II.)

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the Annals of Electricity for October last, will be found a memoir on Marine Lightning Conductors. This memoir is addressed to the British Association, and is considered by Mr. Sturgeon, the author of it, to merit in a high degree the especial consideration of all the learned scientific bodies in Europe and America.

The author endeavours to show, that a metallic rod whilst transmitting a charge of electricity, is always productive of

powerful lateral explosions, not only on near bodies, but on bodies at very great distances. This effect, he thinks, in the case of a lightning rod, is a very fearful circumstance.

2. If this deduction be worth anything, it is altogether subversive of the use of such rods as a means of protection from lightning. I have thought it right, therefore, to examine carefully the experiments and reasonings, which have led the author to this conclusion; and since the inquiry bears materially on a question of great public interest, and contains many new phænomena of electrical action, I hope it may not be considered unworthy a place in your very valuable Journal.

3. Although Mr. Sturgeon has spoken in a slighting way of me and my experiments, and has laboured hard to invalidate them, I still feel, that any personal consideration is comparatively of minor consequence. I will not therefore trouble your readers on the subject. I merely wish to have it understood, that this is not a reply to that large part of the memoir levelled at myself, but is simply an investigation of the author's "Theoretical and Experimental Researches," and of his claims to our confidence as a writer on Electrical Science.

4. So long since as the years 1728 and 1729, Mr. Grey observed the phænomena of electrical conduction and insulation.

(*a*). Thus a metallic ball, J, fig. 1, (Plate II.) supported on the glass rod *g*, is said to be insulated, and if electrified, will cause a spark in the opening between the metallic body B and the ball J.

(*b*). If we connect the ball J with any distant body *c*, by means of a metallic wire as in fig. 2, and electrify it as before, the spark will still occur in the opening at the distant body *c*, the electricity being conducted by the intermediate wire.

(*c*). The distance at which this effect may ensue, is very considerable. Mr. Grey succeeded in making it sensible at a distance of 765 feet*.

(*d*). The effect is most sensible when the body B is connected with the ground, which places it, by a law of electrical action, in the most favourable state for receiving the spark.

5. I am desirous to call especial attention to these results, notwithstanding their elementary character, because, as we shall presently see, they are really nothing more or less than the essence of Mr. Sturgeon's *new researches*, and which he claims to have considered by all the learned societies of Europe and America.

* Priestley's History of Electricity.

6. When we attempt to charge an electrical jar, J, fig. 3, it is observable, that as the charge accumulates on the inner surface, a corresponding quantity of electricity is forced off from the outer, and without this double effect takes place we fail to accumulate a charge.

(*e*). To render this evident, we have only to place the jar on an insulator, as in fig. 3; we shall then find, that for every spark we send into the jar, a similar spark will leave its outside, either from the coating directly, or from any distant body *c* connected with it as in fig. 4.

The outer coating J, therefore, and distant body *c*, may be considered in their insulated state as being insulated conductors under the conditions represented in fig. 2.

(*f*). Suppose the jar charged, and that it remains insulated; then we may discharge it, either by one dense shock through the rod *t*, fig. 4, or gradually, in the reverse way of charging; viz. by continuing to draw sparks from the knob *m*, and add them to the coating J: the circumstance however of our being enabled to take a finite spark, from either side alternately, whilst the jar rests on an insulator, is sufficient to show, that the accumulated electricity is never exactly balanced between the opposed coatings, so that there will always be an excess of either positive or negative electricity over the neutralizing quantities themselves, disposed on the coatings of the jar.

(*g*). When therefore we discharge the jar, this excess of free electricity will speedily expand itself over the outer surface J, the discharging rod *t*, the knob of the jar *m*, or any other body, *c*, fig. 4, connected with it, which, as in the case of the simply electrified conductor, J, fig. 2, will cause a spark to occur in either of those places. The intensity of this spark however will depend on the capacity of the jar. It is *less* with a large jar, and *greater* with a small one, the quantity of electricity discharged being the same.

(*h*). When the jar has been discharged, the knob, the outer coating, and all the bodies connected with it, will be found in the same electrical state. We may make this state either positive or negative, by taking a spark either from the knob or coating previously to discharging the jar.

(*i*). This small spark caused by the excess of free electricity, may be obtained even though the jar be connected with the earth, provided we seize it before the conductors have had time to operate in carrying off the residuary accumulation; Professor Wheatstone having shown by his unrivalled experiments on electrical conduction, that some portion of time elapses in the passage of electricity through wires.

By bringing a metallic ball, B, fig. 3 and 4, therefore in a free state, either very near the discharging rod *c c*, fig. 3, the outer coating J, or any body, *c*, fig. 4, in connexion with it, previously to making the discharge, we seize as it were some of the residuary electricity before it has time to pass off, and hence it becomes evident in this particular direction. The effect, however, will be necessarily greatest when the jar and its appendages are quite insulated. After this spark has taken place, the jar will be found again slightly charged, with what has been called a residuary charge, so that the phænomenon itself is actually the *same as that already* observed in charging the jar originally (*e*).

7. Now these simple experiments (*g*), (*h*), (*i*), are just the experiments described by Mr. Sturgeon, in which he imagines that the small spark above described, is produced by a lateral action of the rod carrying off the discharge. He seems to consider it as a novel and important fact, and calls upon the "principal scientific bodies in Europe and America," and "the ablest electricians the world can produce," in order that it may be fully sifted and explained. He takes great credit for having placed this subject before them in a "*proper* light," and cannot account for the circumstance of my having overlooked it*.

8. But since it is clear that this supposed lateral explosion really resolves itself into one or two simple facts (*a*) (*b*), known to electricians for more than a century since, "the ablest electricians the world can produce," may perhaps be disposed to think such an occupation of their time unnecessary, and the several "Learned Societies in Europe and America" may consider it would have been quite as well for Mr. Sturgeon's credit, as a lecturer on natural philosophy, if he had not troubled them on the occasion.

9. The following is Mr. Sturgeon's version of these experiments:

This kind of lateral discharge, "consists in the displacement of the electrical fluid of bodies vicinal to a continuous conductor carrying the primitive discharge."

Exp.—If a Leyden jar, J, fig. 3, be discharged through a rod *c c*, a spark will appear at the opening *o*, between the metallic body B placed near the rod.

Exp.—If instead of discharging the jar through the rod *c c*, fig. 4, we discharge it by a common discharging rod *t*, still the spark will appear at *o* as before.

* "I mean to submit the substance of my Memoir to the consideration of the principal scientific bodies in Europe and America, in order that the subject may be fully sifted and explained by the ablest electricians the world can produce."—*Ann. of Elect.*, p. 191.

"The effect," he says, "is much increased by connecting the body B with the ground, and diminished to a certain extent by connecting the outside of the jar with the ground." I have produced the spark, he says, between *c c*, and the body B when placed at 50 feet from the *direct* discharge.

"By this kind of lateral discharge," he observes, "a dense spark may be produced when the bodies B and *c c*, fig. 3, are half an inch apart. Though the jar be only of the capacity of a quart, chemical decompositions may be effected by it."

10. Mr. Sturgeon does not state precisely how these experiments were conducted, but the nature of the manipulations would have a material effect on the result. If for example a small jar of a quart capacity were charging from a very powerful machine, and the discharge produced at the time of charging, either by a spontaneous explosion between the balls, *m c*, fig. 3, or by an insulated discharger, then, as is evident, not only would the outer coating and its appendages become charged with the residuary electricity proper to the jar, but also by electricity from the prime conductor, which would assuredly pass over at the instant of the discharge. In Mr. Sturgeon's account of his experiments this fallacious method would appear to have been resorted to. He says, "a spark is felt at every discharge through the circuit represented in the figure," that is *m c c*, fig. 3. Now the continued discharges implied in this statement, could only be produced by continuing to work the machine in connexion with the jar. This circumstance alone would be sufficient to falsify the whole.

11. The following experiments are not unimportant as bearing on the present question.

(*k*). Let a jar, J, fig. 3, be charged positively, removed from the machine, and insulated.—Under this condition discharge it. When discharged, let the electrical state of the knob *m*, discharging conductor *c c*, the outer coating J, or any distant body *c c*, fig. 4, connected with it, be examined; they will all be found in the same electrical state, which state will be precisely that, exhibited by the outer coating and knob, whilst charging, and the small residuary spark will be plus.

(*l*). Charge the jar as before; but before discharging it, withdraw the free electricity from the knob. The electrical state of the coating and appendages will be now changed, and the small residuary spark will be minus.

(*m*). Immediately *after* the discharge, apply a metallic body B, figs. 3 and 4, either to the coating J, or any body connected with it. A residuary spark will be thrown off.

(*n*). Place a metallic body B near the discharger, or outer

coating, previously to making the discharge; the spark will then appear to ensue at the time of the discharge.

(*o*). Examine the jar after this residuary spark has been taken from the outer coating, and it will be found again slightly charged as at first.

(*p*). Charge a jar, exposing about two square feet of coating, with a given quantity of electricity, measured by the unit jar *u*, fig. 5. Let a conducting rod terminating in a ball *r*, project from the outer coating, and place near it the electroscope E*. Discharge the jar through the rod *c c* as before, and observe the amount of divergence of the electroscope. Double the capacity of the jar, and again accumulate and discharge the same quantity. The divergence of the electroscope will be very considerably decreased. Add a second and a third jar to the former, and the effect will be at last scarcely perceptible: connect the jar with the ground, and with a given quantity the spark will vanish altogether.

(*q*). Accumulate a given quantity as before, and observe the effect of the residuary charge on the electroscope. Let a double, treble, &c., quantity be accumulated and discharged from a double, treble, &c., extent of surface; that is to say, for a double quantity employ two similar jars, and so on: the effect will remain the same.

(*r*). The quantity and surface remaining constant, let the discharge be effected by discharging circuits *c c* fig. 3, of different dimensions from a large rod down to a fine wire which the charge in passing can make red-hot. Observe the effect on the electroscope in each case: it will be found nearly the same, being rather less where the tension in the discharging wire is very considerable.

(*s*). Connect the jar with the ground, and place between the discharging conductor *c* fig. 3. and a metallic mass B, a small quantity of percussion powder, inclosed in thin paper. The powder will not be inflamed, even in the case of the discharging conductor becoming red-hot; whereas in passing the slightest spark, it inflames directly.

(*t*.) Insulate a circular conducting disc, M, fig. 6, of four feet in diameter: it may be made of wood covered with tin foil; oppose to it a similar disc, N, connected with the ground. Place a conducting rod, *c c*, on the lower plate, and near it a metallic body, *o*; electrify the upper plate, *m*; dense sparks will fall on the rod, *c c*, but no effect is observable on the vicinal body, *o*, even though percussion powder be placed in the opening.

* The electroscope I employed is described in the Transactions of the Royal Society for 1834, Part 2, page 214. For more accurate measurement we should employ the electrometer, p. 215.

12. These experiments are conclusive of the nature of Mr. Sturgeon's experiments.

Exp. (*k*) (*l*)—show, that the electricity of the spark varies with that of the coatings.

Exp. (*m*)—proves that the spark is readily obtained *after* the discharge has taken place; it is not therefore any lateral explosion caused by the discharging rod.

Exp. (*o*)—proves that the spark is merely a residuary accumulation.

Exp. (*p*) (*q*)—prove that the spark is of different degrees of force, when the electricity is discharged from a greater or less extent of surface, whilst double, treble, &c., quantities, when discharged from double, treble, &c., surfaces, give the same spark. Now as no one can doubt but that the effect of a double, &c. quantity should be greater than a single, &c. quantity, it is again evident that the spark is not caused by any lateral explosion from the discharging rod; it being a well-established law, that the same quantity has the same heating effect on wires, whether discharged from a great surface or a small one, from thick glass or thin; some little allowance being made for the greater number of rods, &c., when the surface is increased by an additional number of jars *. The effect therefore depending on the jar, Mr. Sturgeon had a greater chance with a small jar than with a large one.

Exp. (*r*)—proves that the degree of tension in the rod is not of any consequence.

Exp. (*s*) (*t*)—show, that no kind of lateral action arises during the passage of the charge.

13. Mr Sturgeon confounds this residuary spark, with the Earl of Stanhope's experiments on induction: he observes, p. 176, "Viscount Mahon studied this kind of lateral discharge very extensively." But any one who considers His Lordship's work, will soon detect the fallacy of such a conclusion. Lord Mahon shows, that when an electrical charge is about to pass from a body M, fig. 7, in the direction C L, the action upon a near body N will displace some of its electricity; hence a spark will take place at E between that body and another connected with the ground whenever the discharge takes place from M, in consequence of the return of the displaced electricity. This effect His Lordship termed the "returning stroke." Now to apply this to the operation of a thunder-cloud. Let M, fig. 6, represent a mass of cloud

* Philosophical Transactions for 1834. Part II. p. 225, and Faraday's Researches.

covering a portion of the earth's surface N. Let *c c* be a discharging rod, and *o* some near body. Then by Lord Stanhope's experiment the charged cloud M will displace from the surface N, and all the bodies on it as *c c*, *o*, &c., a portion of their natural electricity, which will again return when the discharge has been effected. The conditions of Lord Mahon's experiment cannot obtain between the conductor *c c* and the body *o*, since they are both in the same forced state *. It is very easy to perceive, that the electrical relations of two bodies *o* and *c between* the boards, is different from that between a conductor J, fig. 1, charged with electricity, and a body B in its natural state; or that of a conductor C, fig. 6, carrying off the displaced electricity of the lower plate N, and a body B neutral. Besides, in Lord Mahon's experiment, fig. 7, the electricity of the return spark is different from that of the primitive charge in M; whereas, in Mr. Sturgeon's experiment, the spark is of the same kind. So little did His Lordship anticipate any objection to the use of lightning rods in consequence of his experiments, that he declares his conviction of their passive operation, and reproves those who "ignorantly conclude" that they are of a dangerous nature.

14. We have been here discussing what the author calls a *third* kind of lateral discharge; but he mentions a *first* and *second* kind also. The first kind, he says, "takes place at every interruption of a metallic circuit;" "it displaces loose bodies," &c. This is evidently the effect of mechanical expansion, and is the very effect we avoid by means of a lightning rod. He alludes to Dr. Priestley as authority on this point; how unfortunate for his whole doctrine! Let us consider for a moment what Dr. Priestley says: "That the cause of this dispersion of bodies in the neighbourhood of electrical explosions is *not their being suddenly charged with electric matter*, is, I think, evident. I never observed the *least attraction of these bodies toward the brass rods, through which the explosion passed*, although I used several methods which could not fail to show it. I even found that the explosion of a battery made ever so near a brass rod, did not so much as disturb its electric fluid; for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found the balls were not in the least moved†."

* This applies to Mr. Sturgeon's Exp. (9.)—If B fig. *c*, 3. were on the same insulation with the jar J and rod *c*, *no spark could occur at o*, except by a division of the charge, whatever quantity passed through *c*. This fact alone is conclusive of the point in question, proving clearly that the spark is *not* a lateral explosion.

† The reader will distinguish here between this experiment and Lord

We have seen how little support Mr. Sturgeon derived from Lord Mahon; he obtains still less from Priestley, who, without any compromise, sweeps away his whole theory. Lord Stanhope and Dr. Priestley, eminent amongst the philosophers of their day, will be doubtless admitted to be as good authority as Mr. Sturgeon.

15. The *second* kind of lateral discharge is, we are informed, "a radiation of electric matter from conductors carrying the primitive discharge." It takes place, the author says, from edges, and that hence "sharp edges of metal carrying a flash of lightning would discharge necessarily a great quantity of fluid into neighbouring bodies." No author is pressed into the service on this occasion, and for the best possible reason, no accredited writer has ever treated of such a phænomenon as applying to a lightning rod. It is in fact applicable only to charged conductors. Thus ragged or pointed rods attached to the prime conductor of the electrical machine exhibit brushes of light, whilst other similar bodies, within their influence, have the appearance of stars. The lights on steeples, and on the sail yard and masts of ships, mentioned by Pliny, are of this kind. Franklin explained these phænomena, and showed that pointed bodies were favourable to the rapid dissipation of electrical accumulations, and, as is well known, availed himself of the important fact in his application of the pointed lightning rod. How Mr. Sturgeon has contrived to associate this effect with the effects of discharges of lightning *through* conductors it is difficult to say. It is certainly a very strange confusion of things. That the effect in question has nothing to do with a sharp or round edge, or angular discharges, may be shown by the following experiments:—

(*u*). Dr. Priestley discharged a battery over a wire circuit perfectly straight, and also over the same circuit passed about pins so as to make sharp angles:—the result of the charge on fusing a given length of wire was not influenced, which could hardly have been if the angular portions had thrown off or discharged into the neighbouring pins, &c. any of the charge, it being well known that the least diminution of quantity is fatal to a delicate experiment on the fusion of wire.

(*v*). Discharge a given quantity of electricity by a continuous rod free of edges, through a wire passed through the ball

Mahon's. The latter relates to the influence of a permanently charged conductor on a body neutral; whereas Priestley's applies to the action of wires carrying vanishing quantities of electricity, the very essence of Mr. Sturgeon's experiment. Dr. Priestley would not have told us, had he brought his rod near the *free side* of his battery, that then the pith balls were not moved.

of an air thermometer, and also by a similar rod with ragged edges, placed near other metallic masses: the effect on the wire remains unchanged *.

It is not difficult to perceive the distinction of the two cases just alluded to. If Dr. Priestley had *insulated* his wire, and then charged it in the ordinary way, brushes of light would doubtless have escaped from the angular portions; whereas the wire when acting as a discharging circuit can exhibit no such appearance. The electricity is then evanescent, and by a law of electrical action determined rapidly toward the negative surface. Many facts might be adduced conclusive of this point, but it seems scarcely worth while to dwell longer on it.

16. The great end which the author proposes to himself in this memoir, is an exposition of the danger attendant on my method of fixed lightning conductors for ships, successfully tried in the British navy for upwards of ten years;—with a view to a substitution of an untried method of his own. It may be worth while, therefore, in conclusion, to see whether the objections he so strongly insists on, do not equally apply to his own conductors as well as to mine, and, in short, to lightning conductors generally.

17. In the first place, he tells us (see 191.) "that it is possible for the most spacious conductor that can be applied to a ship to be rendered sufficiently hot by lightning to ignite gunpowder."

18. In the next place, he says, (202.) that the "lateral discharge will *always* take place when the vicinal bodies are capacious, and near the principal conductor or any of its metallic appendages." This was the case, he says, when only his small jar was used, and with this small jar he could produce lateral discharges at a distance of fifty "feet from the direct discharge."

19. Thirdly, he tells us (203.) that "the magnitude and intensity of a flash of lightning being *infinitely* greater than anything which can be produced artificially, the lateral discharges must be *proportionally greater*," that is to say *infinitely* great.

20. Taking these data as true then, it follows that any lightning conductor carrying a flash of lightning, would at an *infinite* distance, produce a lateral explosion *infinitely* great, and of course do an *infinite deal* of mischief. Hence, every powder magazine having a lightning conductor, every ship with a lightning chain in her rigging, should whenever lightning struck the conductor be destroyed; for in no case is the

* For a description of this instrument, termed an electro-thermometer, see Transactions of the Royal Society for 1837, p. 18.

conductor at one third the distance from the inflammable matter, of that, at which Mr. Sturgeon can produce a lateral discharge with a jar of "only a quart capacity," viz. "50 feet."

21. But Mr. Sturgeon proposes to apply cylindrical copper rods in the rigging; their "upper extremities to be attached to the tops, &c. &c.", "their lower extremities to the chains of the shrouds," and to be united by "broad straps of copper to the sheathing," that is to say, by conductors with edges, which he says throw off the charge into neighbouring bodies; this too after having told us, that the most spacious conductor may become red-hot, and that lateral discharges *always* take place when the vicinal bodies are *capacious*, and near the principal conductor or *any of its metallic appendages.* Under such circumstances what is to become of the rigging, sails, masts? will they not be set on fire? Are not the massive iron hoops and other metals about the masts, the chains of the shrouds bolted through the ship's side, and other metallic bodies in the hull, such as bolts, tanks, chain cables, &c. &c. *vicinal capacious bodies*, and reaching by interrupted metallic circuits up to the very magazines Mr. Sturgeon talks so much about? Must not a ship with such conductors be necessarily destroyed? Surely he must give the British Association and the learned bodies of Europe and America, &c., very little credit for philosophical penetration, if he thinks they will not immediately discard such philosophy as this.

22. Either his "theoretical and experimental researches" are true, and his system of conductors fatal and absurd, or otherwise, if his conductors be good for anything, then his theoretical and experimental researches are good for nothing. He may adhere either to the one or the other, but he cannot have both; such is the *reductio ad absurdum* in which he is involved.

Mr. Sturgeon's anxiety to arrive at conclusions unfavourable to my conductors, has led him to conclusions subversive of *all* conductors, his *own especially.*

23. The mere circumstance of finding his "*third* kind of lateral explosion" decrease in power, by uninsulating his jar, might alone have led him to doubt the accuracy of his deduction. On so important a point, and before he ventured to awaken the prejudices and fears of the uninformed, we had a right to expect at his hands a profound scientific inquiry. He should at least have tried whether he could not get this spark after the main charge had passed (*m*) as well as at the *apparent* time of passing. The quantity of electricity should have been accurately measured, and its effects in producing

the spark determined, both in relation to the quantity and surface over which it was distributed (*p*). The form and dimensions of the discharging conductor should have been varied (*r*). The final electrical state of his apparatus, as also the electricity of the spark, should in common prudence have been examined (*k*), together with other manipulations quite inexcusable to neglect on such an occasion. He has, however, failed in everything calculated to give value to his inquiries, as I think has been fully shown. They are hence not entitled to the smallest confidence, and it is not a little extraordinary that he should have done so, whilst taking credit to himself for *superior sagacity*, and an acquaintance with facts of which he says I did "not seem to be aware," e. g. the most common-place facts in electricity.

24. In conclusion, I have no hesitation in giving it as my confirmed opinion, after a long and severe examination of the laws of electrical action, and of cases of ships and buildings struck by lightning;—that a lightning rod is purely passive, that it operates simply in carrying off the lightning which falls on it, without any lateral explosive action *whatever*. I do not deny the general inductive effect mentioned by Lord Stanhope on bodies opposed to the influence of the thunder-cloud, and that the displaced electricity will again find its equilibrium of distribution, and return to those bodies, which effect would necessarily take place, whether we had a lightning rod or not (13); an additional reason for linking the detached conductors in a ship's hull into one great mass, so as to have as few interrupted circuits as possible in any direction.

This opinion, by the citation of a few striking cases in which ships have been struck by lightning, I hope in a future paper fully to substantiate, should you think the subject of sufficient consequence*.

Appendix.

The author, probably perceiving how little he had gained by quoting Lord Mahon and Dr. Priestley, observes, in a supplementary note, page 235, "Perhaps the experiments of Professor Henry would be more to my purpose." These experiments, however, are no more to "his purpose" than the others, as any one may see who will examine the Professor's communication, in the seventh report of the British Association, page 25. The experiments there described relate to minor electrical discharges, similar to those already mentioned (*i*). These were obtained by throwing simple sparks from an electrical machine, on small wires or rods, either insulated or connected with the earth: the wires became lumi-

[* We shall be most happy to receive and insert any further communication from Mr. Harris.—Edit.]

nous and the rods emitted sparks. In this case, as Professor Henry observes, the electricity of the machine must be considered as free electricity; and as the bodies on which they fell were all in their natural state, the spark is immediately thrown off as a lateral discharge. Whether insulated or not, the electricity of the body is evidently acted on by induction, before the spark can be distributed over it or the earth. Hence, when sparks of about an inch long are thrown on the upper end of a lightning-rod, or other metallic body passing into the earth, the induction upon the rod and earth requiring a short time for its development, a spark is thrown off upon any adjacent conductor in a state to receive it. Such experiments, therefore, apply only to small quantities of electricity suddenly thrown upon conductors in a neutral state. This, as I have shown, (13, figure 6,) is a distinct case from that, in which a charged surface throws off its redundant electricity upon an opposite surface eager to receive it through a conducting-rod sharing in the electrical state of that surface, and which is consequently prepared already by induction to discharge it. One might be led to infer, from the particular description given by the author of this experiment, page 235, that sparks had been obtained from a lightning-rod at the time of its conveying a discharge of lightning. It may not be amiss to add, that Professor Henry did not consider these experiments as applicable to lightning-rods; and that in accordance with the opinion of Biot, he thinks the spark observable at the time of discharging a jar—that is, Mr. Sturgeon's *new* fact —is entirely owing to a small quantity of redundant electricity always existing on one side of the jar, as I have already stated, (*f*,) *and not to the whole charge*.

I am, Gentlemen, yours, &c.

Plymouth, Nov. 5, 1839. W. Snow Harris.

LXXII. *Notices respecting New Books.*

Remarks on the Classification of the Different Branches of Human Knowledge. By J. W. Lubbock, *Esq., Treas. R.S., &c., Vice-Chancellor of the University of London. Second Edition.* London and Cambridge, 1839. 8vo. pp. 42. and xxii.

The advancement of knowledge and the continually increasing number of those who apply themselves to its improvement and extension, are daily rendering the subject of classification, whether as relating to the objects of nature or to the different branches of science and learning, more and more important. *Arrangement*, considered as forming itself an object of science, has not been long attended to in this country, even with respect to those branches of na-

tural history,—the sciences of botany and zoology,—in which it is obviously of the highest moment: with regard to the domains and subjects of inorganic nature, its introduction is of very recent date indeed; while, in relation to the various departments of human knowledge itself, it is yet almost unknown among us.

On these accounts, that a second edition of Mr. Lubbock's "Remarks" on the subject last adverted to has been called for, is a circumstance at which we may rejoice as indicating that the attention of men of letters and of science has fairly begun to be directed to its interest and value, and to the improvement which it confessedly requires.

As we omitted to notice the first edition, we shall now, after enumerating the contents of the work, state briefly the object which the author has in view, and extract his own modified classification of the different branches of knowledge.

"System of Bacon.—System of D'Alembert.—System of Locke.—System of Bentham.—System of Chambers.—System of Ampère.—System of Comte.—System of the Encyclopædia Metropolitana.—System proposed by the Author.—Classification of Books proposed by the Author.—Classification of the distinguished Men of modern times.—Societies for the promotion and advancement of Science.

"Fr. Baconi Partitio universalis Doctrinæ Humanæ.—Système figuré des Connaissances Humaines, from D'Alembert's Works, vol. i. p. 333.—Division of the Intellectual Faculties, from the *Manuel des Aspirants au Baccalauréat-ès-lettres*.—Chambers's Divisions of Knowledge.—Classification des Connaissances Humaines, by Ampère.—System of M. Destutt Comte de Tracy.—Tableau Synoptique of M. Comte.—Plan of the Encyclopædia Metropolitana.

"*Classification of Books*.—Idea Leibnitiana Bibliothecæ ordinandæ contractior.—Table Méthodique of Brunet.—Library of the London Institution.—Library of the Royal Institution.—Nouveau Système Bibliographique.—Library of Queen's College, Cambridge.—Classification from the Edinburgh Review.

"Original Classification of the Institut Royal de France.—Present Classification of the Institut Royal de France."

Mr. Lubbock commences by alluding to the universal acknowledgement, that in consequence of the progress of science, the divisions which were formerly proposed by Bacon and D'Alembert are not suited to its present condition. He then cites the critical examination of their labours by Dugald Stewart, which terminated, unfortunately in his judgement, only in a conviction that the logical views of those philosophers on the subject are radically and essentially erroneous.

After remarking, further, that "the classification of human knowledge is intimately blended with the classification of literature," and expressing his opinion "that Dugald Stewart may not have given attention to the assistance which can be derived in the consideration of the former question from the labours of Leibnitz, Brunet, Mr. Horne, and others, who have considered specially the latter question," Mr. Lubbock reviews and discusses, concisely, it is true, but in a very explicit and definite manner, the classifications of human knowledge or of books, severally proposed by Bacon, D'Alembert, Leibnitz, Locke, Cousin, Bentham, Chambers the Encyclopædist, Ampère, and Comte, including also "the system of the Encyclopædia Metropolitana," which latter it may perhaps add to the interest of the

subject to state, although a circumstance not adverted to by Mr. Lubbock, was devised by Coleridge. Mr. Lubbock next proceeds to explain his own system of classification of the different branches of human knowledge, as given at the conclusion of the present article; in the course of which explanation he introduces the following important remarks :—

"The subject is not without difficulty; in matters of this kind, which like all questions of taste, admit of various solutions, it is impossible to lay down imperative rules; I at least have no such pretension. Still I contend that to all, but especially to those concerned in education, the contemplation of any rational classification of the branches of human knowledge must be useful. To those who habitually confine their attention to the limited speculations of any particular science, a more extended survey of the relations which it bears to others cannot fail to be profitable. I have endeavoured to adhere closely to established scientific nomenclature; by so doing I have avoided the necessity of introducing extended definitions and explanations of the subjects which are embraced by different sciences or subdivisions of the principal categories, and which are the less wanted here, because those who write complete treatises upon any branch of human knowledge, generally commence by a careful limitation of the objects which are included in it. The limits of the subdivisions are generally ascertained, but the line of demarcation between NATURAL PHILOSOPHY and NATURAL HISTORY has not been sufficiently considered, and is by no means well defined. I think, perhaps, NATURAL HISTORY might be advantageously limited to the study of organized beings, including Botany and Zoology, and NATURAL PHILOSOPHY (*La Physique*) might then be restricted to the study of inorganic and terrestrial phænomena as described and limited by Prof. Lamé. Although there are some points which may be considered doubtful, yet these do not appear to me to be numerous, or the difficulties insurmountable which stand in the way of obtaining an unexceptionable system of classification, which should meet with general adoption. In the present state of this important problem, I have thought it might be useful to place in the Appendix various schemes of classification which have been devised, so that by their juxtaposition their comparative merits may be readily investigated*, my object being to bring under notice facts bearing upon the subject, in the hope that, divested of fantastical and unnecessary innovations, the question may find more favour than it has hitherto experienced."

Having in conclusion of this part of the subject briefly compared his own system with those of Ampère and the Encyclopædia Metropolitana, he proceeds to consider the difficulties which occur in the distribution of books, and their causes, noticing the classifications of books by Leibnitz, Girard, Brunet, Mr. Horne, and some others, and introducing his own scheme, founded upon that of the branches of knowledge themselves, as above mentioned, by the following apposite observation : "No system of classification of the branches of human knowledge can be complete which will not bear the test of its application to the classification of books; and conversely every classification of books which is not purely artificial, involves a classification of the subjects of which they treat."

Mr. Lubbock next compares his own classification of books with several others, adverts to the distribution of the objects of science and literature by and among universities and scientific bodies and societies, and concludes the text of his little work with the following reflections :—

* See a note in the introduction to the *Œuvres Philosophiques de Bacon*, by Professor Bouillet, in which some other systems which have been proposed are mentioned.

"Sufficient reasons might perhaps be adduced to explain the irregular distribution of the objects to which different learned bodies are devoted, and the absence of societies specially instituted for the cultivation of some branches of science. The cooperation of individuals is more practicable in some cases than in others, and the publication of valuable papers on some technical subjects, not of general interest, would be much curtailed if the expense were always left to fall upon the authors.

"It is more difficult to offer satisfactory reasons for the absence of method, and for the disproportionate attention given to some subjects in many schemes of elementary education. In the great public schools of England the time of the student is still almost entirely confined to the study of the Greek and Latin writers; and the difficulties which stand in the way of the introduction of other branches of knowledge have yet to be surmounted. Still, as in the days of Milton, 'We spend seven or eight years merely in scraping together so much Latin and Greek, as might be learned otherwise easily and delightfully in one year; and that which casts our proficiency therein so much behind, is our time lost, partly in too oft idle vacancies given both to schools and universities; partly in forcing the empty wits of children to compose themes, verses, and orations.' It appears to me that some general notions respecting the different branches of human knowledge, embracing at least their definitions, should form a chapter in all elementary education."

The contents of the Appendix have already been stated.

We strongly recommend Mr. Lubbock's views to the attentive consideration of philosophers, of literary and scientific men in general, and also to that of bibliographers and librarians. The following is his "System of Classification of the different branches of human knowledge," already adverted to:—

History. CIVIL HISTORY; Statistics. History. Biography. Antiquities. Numismatics. Diplomacy. Genealogies. Heraldry.—GEOGRAPHY; Geography. Hydrography. Voyages and Travels.—HISTORY OF THE ARTS, OF THE SCIENCES, AND OF LITERATURE. ECCLESIASTICAL HISTORY. SACRED HISTORY.

Philosophy.—RELIGION. Revealed Religion. Natural Religion.—JURISPRUDENCE; Common and Civil Law. Law of Nations. Statute and Common Law. —INTELLECTUAL, MORAL, AND POLITICAL PHILOSOPHY; Metaphysics. Ethics. Education. Political Economy.—LOGIC (including LANGUAGE); Logic. Art of Writing. Hieroglyphics. Art of Printing and Engraving. Mnemonics. Grammar. Rhetoric.—MATHEMATICS; Pure Mathematics. Astronomy. Mechanics. Optics.—NATURAL PHILOSOPHY; Electricity. Magnetism. Galvanism or Voltaic Electricity. Chemistry. Meteorology.—NATURAL HISTORY; Mineralogy. Geology. Agriculture. Botany. Zoology.—MEDICINE; Physiology and General Anatomy. Comparative Anatomy. Materia Medica and Pharmacy. General Pathology and the Practice of Medicine. Surgery. Medical Jurisprudence *.—[ARTS, TRADES, AND MANUFACTURES; Rural and Domestic Economy. Manufactures.]—*The Fine Arts.*—Architecture. Sculpture. Painting. Music. Poetry.

LXXIII. *Proceedings of Learned Societies.*

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At a meeting of this Society on the evening of Monday the 11th of November (Dr. Hodgson, the President, being in the chair), Mr. Whewell explained a new theory of the tides. The phænomena of the tides have hitherto been referred to the *equilibrium theory*, the elevation of the waters which occasions the tide being compared

* By many *Forensic Medicine* is considered a better title for this branch.

with the elevation which the moon would produce in the ocean if the earth and moon were both at rest. But the general motion of the waters of the ocean does not countenance this theory, or allow us to suppose that a fluid elevation resembling that of the equilibrium spheroid *follows* the moon from east to west; for the Pacific, the largest ocean, has no tide in its central parts; and at its eastern shore, near Cape Horn, the tide wave runs from west to east, although there is nothing to prevent its following its natural course.

The new theory which was offered was this:—The tide of each large ocean may be considered as nearly independent of the tides of other waters. The central area of each ocean is occupied by a *lunar* wave, which oscillates, keeping time with the moon's returns, and having its motion kept up by the moon's attraction acting at each return. From the skirts of this oscillating central area, tides are carried on all sides by *free* waves, the velocity of which depends upon the depth and local circumstances of the sea; and thus the *litoral* tides may travel in any direction, while the *oceanic* tides near the centre of the oscillating area may be small or may vanish altogether.

This theory was confirmed by a reference to tide observations on the eastern and western sides of the Pacific, and by mathematical calculations tending to show that such a motion is mechanically possible. It was remarked that single observations can be of small use in deciding upon such a theory; and that it can be judged of only when we have observations numerous enough to enable us to draw the *systems* of cotidal lines which belong to the shores of the Pacific. With this view it is very desirable to obtain numerous and connected observations of the tides on the eastern shores of Australia, the Indian Archipelago, the Philippine Isles, the Loo Choo Isles, and Japan.

This theory to a certain extent coincides with the views respecting the tides published by Capt. Fitz Roy in his appendix to the voyages of the Adventure and Beagle.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

Twenty-sixth Annual Report of the Council.

"In presenting their twenty-sixth Report, the Council have again to express their regret that the Fifth Volume of the Society's Transactions, which was promised at the last Meeting, has not yet been completed. The delay is principally owing to the Editor, and to circumstances over which he had no control. They, however, believe that it will be published early in the next year.

"They have recently had to deplore the loss of two of the Founders, and most munificent Members of the Society, in the death of the Earl of Mount Edgcumbe, and Sir John St. Aubyn, Bart., who had frequently filled the offices of Vice-Presidents.

"During the past year the additions to the Museum have been very considerable: among the most important contributions they particularize a series of specimens principally from the neighbourhood of Hull, which contains also many from other localities, both

British and foreign, from John Edward Lee, Esq., corresponding member of the society; a collection of shells from the tertiary formations on the skirts of the Alps from the Chevalier Michelotti, of Turin; and a series of rock specimens and organic remains from Pembrokeshire from Henry MacLauchlan, Esq., F.G.S., which affords an instructive and admirable illustration of that region, and shows the intimate connection between it and the fossiliferous rocks of Cornwall.

"The library has been enriched by the Transactions of the American Philosophical Society from its commencement, obtained in exchange for a set of the Society's Transactions; and by the purchase of Goldfuss, Deshayes, and Brongniart's admirable works on fossil geology.

"The Council have therefore great pleasure in congratulating the Society on the flourishing state of its collections. As a large proportion of them are deposited in drawers, they consider that additional space ought, if possible, to be obtained for their display, and in order to render them more generally available and useful. In consequence of this augmented and rapidly increasing value of the Society's property, they advise that, for the future, it should be insured in the sum of 1000*l.* instead of 400*l.* as heretofore.

"The establishment of an Academy for instruction in the collateral branches of the Science of Mining is an object which has occupied the attention of the Society from its foundation, and its necessity and importance have been often the theme of its reports. The Council congratulate the Society on the accomplishment of this most useful and desirable object by the munificence of Sir Charles Lemon, Bart., and on the ability and reputation of the gentlemen who have been selected to carry it into operation. Its establishment is, however, too recent to admit of an opinion as to its adaptation to the wants of our labouring population: still the Society cannot allow the completion of one of the earliest wishes of its founders to pass without an expression of their most ardent hopes for its success."

The following papers have been read since the last Report. On a formation of bog iron ore at Perran Consols Mine. By W. Mansel Tweedy, Esq., F.H.S., Member of the Society.—Notes accompanying a series of specimens from some parts of Pembrokeshire. By Henry MacLauchlan, Esq., F.G.S., Corresponding Member.—Notice of the discovery of organic remains in the quartzose slate of Gerrans-bay. By Charles W. Peach, Esq., Associate.—On the mineral composition and mechanical structure of the metalliferous veins of Cornwall, and their relations to the rocks they traverse. By W. J. Henwood, F.G.S., London and Paris, Hon. M.Y.P.S, Secretary and Curator.—An account of the Quantity of Copper produced in the United Kingdom, in the year ending 30th June, 1839. By Alfred Jenkin, Esq.

At the Anniversary Meeting held on the 4th October, 1839, Davies Gilbert, Esq., D.C.L., F.R.S., President, announced that the following Gentlemen had been elected since the last Report.—*Honorary Members.*—M. Becquerel, President of the Academy of

Sciences, Paris. The Right Rev. the Lord Bishop of Norwich, P res L.S., F.G.S., &c. John Prideaux, Esq., Plymouth. *Corresponding Members.*—Richard Thomas, Esq., Civil Engineer, Mellingye, near Falmouth. Jonathan Couch, Esq., F.L.S., Polperro. *Ordinary Members.*—W. Beal, Esq., LL.D., &c., Tavistock. Carteret J. W. Ellis, Esq., A.M., Trengwainton. Seymour Tremenheere, Esq., London. *Associate.*—Captain Thomas Tiddy, Mona Mine, Anglesea.

Officers and Council for the present year. *President.*—Davies Gilbert, Esq., D.C.L., V.P.R.S., &c. &c. *Vice-Presidents.*—John Batten. John D. Gilbert, F.R.S. Thomas Robins. Francis Rodd. *Secretary and Curator.*—W. J. Henwood, F.G.S., London and Paris. *Treasurer.*—Joseph Carne, F.R.S., F.G.S., &c. *Librarian.*—Richard Hocking. *Council.*—John S. Enys, Assoc. Inst. C.E. Alfred Fox. Samuel Higgs. George D. John. Richard Millett. Samuel Pidwell. John Richards. Rev. Canon Rogers. Benjamin Sampson. James Trembath, Jun. W. Mansel Tweedy, F.H.S. Michael Williams.

ROYAL SOCIETY OF EDINBURGH.

Memorandum on the Intensity of Reflected Light and Heat. By Prof. Forbes.* " At the meeting of the Society on the 4th February, I remarked, on the occasion of Professor Kelland's paper on the intensity of reflected light, that it was almost without a parallel in science, that a quantitative physical law like that of the intensity of the reflection of light at different angles, should have first been divined by the rare sagacity of Fresnel, and confirmed by the very different but elaborate mathematical investigations which Mr. Green of Cambridge and Professor Kelland have applied to the subject, whilst scarcely any attempt has been made towards its verification by direct experiment.

" Some critical cases for polarized light were indeed assumed as the basis of the original formula; and M. Arago has confirmed it it by one or two intermediate photometrical experiments; but the chief evidence for the truth of this remarkable law rests on the indirect observation of the change of the plane of polarization of an incident ray after reflection.

" It occurred to me, about the end of 1837, that the anomalies of photometrical observations being nearly as unsatisfactory as ever, some light might be thrown upon this important subject by ascertaining the law in the case of heat, the intensity of which we have no difficulty in measuring. And since the discovery in the case of heat, of refraction, single and double, of polarization, of total reflection, and the change it produces on polarized light, as well as the change of the plane of polarization by simple reflection, there seems the greatest reason to suppose that the laws of reflection for heat and light, if not identical for both, would be connected by some simple analogy.

" Accordingly, during the month of December 1837, I made some preliminary observations, which encouraged me to proceed. It ap-

* From the Proceedings of the Royal Society of Edinburgh, March 18, 1839.

pears to me, from those observations, that the quantity of heat reflected from different transparent bodies is independent, or nearly so, of the nature of the source of heat, and the diathermancy of the reflecting body, and that at 55° of incidence the intensity of reflected heat is nearly that which Fresnel's theory gives. The substances, however, were not all prepared so as wholly to exclude the action of second surfaces.

"I have this winter resumed the subject. I have had an apparatus constructed for securing sufficient accuracy in determining the angle of incidence, and I have used reflecting surfaces, both transparent and metallic: the former are wedges of plate-glass, by means of which reflection from the first surface only may be observed, and the latter are plane specula of steel and silver. The prosecution, however, of these apparently simple experiments has been attended with unforeseen difficulties; and although the relative proportions of heat at different angles of incidence are now pretty well determined for glass in several cases, I am not prepared to say whether the absolute amount is exactly the same as Fresnel's formula would give, assigning to heat its proper refractive index. It is satisfactory, however, to know, that the approximation to it is much greater than direct photometrical measures have yet given, with the single exception of two experiments of M. Arago already referred to; and that I have reason to believe that the experimental law which Mr. Potter has given from direct observation in the case of light, represents my results much less accurately than the theory of Fresnel.

"With respect to reflection at the metals, I believe I may assert that I have verified the remark of Mr. Potter, that metallic reflection is *less* intense at the higher angles of incidence. I have attempted to ascertain whether it reaches a minimum, and then increases up to 90° of incidence, as Mr. Maccullagh supposes, but I have not obtained decisive results. The quantity of heat reflected by the metals is so much greater than Mr. Potter's estimate for light, as to lead me to suspect that his photometric ratios are all too small, which would nearly account for their deviation from Fresnel's law.

"The most complete verification of Fresnel's law would be found in observations made on heat polarized in opposite planes. These I have attempted, and, so far as they go, they seem to confirm the analogies of heat and light. But the intensity is so much reduced in the process of polarizing, that I fear we must wait for yet more delicate instruments to measure it. In the mean time I may state the method which I propose to employ.

"When heat is polarized by transmission through inclined mica-plates, the polarization is *incomplete*; but if the plates be inclined at the polarizing angle, the transmitted heat is undoubtedly composed of a portion p polarized (in a plane which we will call $+$), and a portion $1-p$ unpolarized. This latter part philosophers are content to consider as compounded of a part $\frac{1-p}{2}$ polarized $+$, and

an equal part polarized —. Now, let the intensity of reflected heat polarized in the plane of reflection (or —), and that perpendicular to it (or +), be represented by the following table, which contains the quantities to be *found.*

TABLE I.

Incidence.	Reflected Rays Polarized.	
	+	—
0°	a_0	b_0
10	a_1	b_1
20	a_2	b_2
&c.	&c.	&c.

Also let the quantities of heat actually *observed* to be reflected after *incomplete* polarization by passing through a mica bundle, be the following:

TABLE II.

Incidence.	Polarizing Plane of Mica Bundle.	
	+	—
0°	A_0	B_0
10	A_1	B_1
20	A_2	B_2
&c.	&c.	&c.

Now, the quantities A and B will be thus composed: The quantity of heat transmitted by the mica bundle and incident on the reflecting surface contains, when the plane of polarization is perpendicular to the plane of reflection, a portion of heat $p + \frac{1-p}{2}$ polarized +, an a portion of heat $\frac{1-p}{2}$ polarized —; let these quantities be m and n; then $m + n = 1$. Now, let the plane of polarization be turned round 90°; then the part polarized in the plane of reflection will now be m, and that perpendicular n. So that the heat is first composed of a part m reflected according to the law of a in Table I., and a part n reflected according to the law of b; and then the converse; so that

$$A_q = m\,a_q + n\,b_q$$
$$B_q = n\,a_q + m\,b_q$$

Hence $A_q + B_q = a_q + b_q$, as it evidently ought, and

$$A_q - B_q = (m-n)\,(a_q - b_q)$$

Hence *the differences of the columns in Table II. are in a constant ratio to the differences of the columns in Table I.*; and as Table I. may be computed from Fresnel's formulæ

$$\frac{\tan^2(i-i')}{\tan^2(i+i')}, \quad \text{and} \quad \frac{\sin^2(i-i')}{\sin^2(i+i')},$$

the agreement or discrepancy will be apparent, and the coefficient $(m-n)$ will indicate the polarizing power of the plates. Also, since the sum of the numbers must be the same for both tables, a single comparison would suffice to determine the index of refraction, which must be assumed in computing the first table."

LXXIV. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF CASEUM.

M. VOGEL employed the method adopted by Berzelius for the preparation of this substance, which consists in isolating from milk by dilute acids.

As the great quantity of fatty substances contained in milk occasions many difficulties in the washing and purification of caseum, M. Vogel made use of buttermilk, which is more completely deprived of the greater part of the butter than mere skimmed milk.

The buttermilk was treated with dilute sulphuric acid; with this the caseum combined, and was afterwards precipitated from it in the state of a white magma: the serum was separated by straining; the compound of sulphuric acid and caseum was agitated and digested with distilled water, and then washed on a filter. This manipulation was repeated, until the washings evaporated on platina foil left no residue.

After the washing, the compound was diffused in distilled water, agitated, and digested with carbonate of barytes, in order to combine the barytes with the sulphuric acid, and dissolve the caseum in water. The liquor was filtered, and the aqueous solution evaporated to dryness in a water-bath. The residue of the evaporation was a yellowish white mass, transparent, and resembled gum arabic in its property of giving a mucilaginous liquid with a small quantity of water. The dry mass was pulverised, digested with æther, to remove the last traces of fatty matter, and then dried at 212°.

A small quantity was calcined in a porcelain capsule, and the complete incineration was aided by the addition of concentrated nitric acid drop by drop; the ashes amounted to the enormous quantity of 21·454 per cent.; these ashes consisted of phosphate and carbonate of lime, and some carbonate of barytes, which had been accidentally introduced.

Omitting the ashes, two analyses gave

	I.	II.
Carbon	51·86	52·53
Hydrogen	7·36	7·82
Azote	16·01	16·20
Oxygen	24·77	23·45
	100·	100·

M. Vogel remarks that albumen, fibrin, and caseum possess several properties which are very nearly allied; all three may exist in two states, dissolved and coagulated, with this difference, that fibrin coagulates of itself as it issues from bodies, that caseum is coagulated in a manner not hitherto understood by pressure, and that the coagulation of albumen is especially produced by heat. All three also possess the property, in the state of coagulum, of dissolving when heated in an excess of concentrated hydrochloric acid, and yielding a fine lilac-coloured solution, a property which may be

advantageously employed, to determine by a simple proof the presence of one of these substances in a coagulated state.

If the elementary composition of these three bodies be regarded independently of the ashes, it will be observed that their composition is almost exactly the same; this is shown by the following table:

	Albumen.	Fibrin.	Caseum.
Carbon	53·08	53·76	52·53
Hydrogen	6·92	7·27	7·82
Azote	16·78	18·59	16·20
Oxygen	23·22	20·38	23·45
	100·	100·	100·

M. Vogel expresses his belief that the numerous analyses which have been cited, lead to the important conclusion already drawn by M. Mulder respecting albumen and fibrin, namely, that albumen, fibrin, and caseum have in the animal kingdom, the same intimate relation that sugar, starch, and gum have in the vegetable kingdom; this observation if followed out, promises to give the most interesting and important explanations respecting a great number of the phænomena of animal organization.—*Journal de Pharmacie*, September, 1839.

ON SULPHATE OF CHLORIDE OF SULPHUR. BY H. ROSE.

Some attempts which were made to convert the sulphate of chloride of sulphur, $S\,\underline{Cl}^3+5\dddot{S}$, by taking sulphuric acid from it, into a compound analogous to the chromate of chloride of chromium, $Cr\,\underline{Cl}^3+2\dddot{Cr}$, did not yield satisfactory results.

If chloride of sodium be treated with sulphate of chloride of sulphur, they unite to form a solid transparent mass, which does not emit fumes, but which when heated yields a vapour having a strong smell of chlorine, and which nevertheless is undecomposed sulphate of chloride of sulphur, containing free chlorine. If it be submitted to distillation, the latter escapes as soon as ebullition occurs. By the decomposition of the chloride of sodium sulphurous acid is eventually disengaged, and there remains a mixture of supersulphate of soda and undecomposed chloride of sodium.

The sulphate of chloride of sodium [sulphur?] decomposes at a temperature much above its boiling point. If the vapour be conveyed through a glass tube heated to redness, chlorine gas as well as sulphurous acid are evolved, but the presence of the latter is not discoverable on account of that of the former. The liquid obtained, after numerous researches into its nature, appeared to be a solution of anhydrous sulphuric acid in sulphate of chloride of sulphur, in which the first partly crystallizes by cooling. It is not possible to procure by this process, a definite compound of chloride of sulphur and sulphuric acid.

The specific gravity of the vapour of sulphate of chloride of sulphur, was found by a mean of five trials to be 4·481. Supposing it to be a compound of 6 volumes of chlorine representing 3 double atoms, and 2 volumes of vapour of sulphur, corresponding to 6

atoms, and 15 volumes of oxygen, the calculated density would be 44·489 or 10 × 4·489. This result agrees well with the experiments, when it is considered that in this compound 23 volumes are condensed into 10. Each atom of the compound corresponds to 10 volumes of vapour.

M. Walter has proposed to consider the chromate of chloride of chromium as a chromic acid, in which an atom of oxygen is replaced by an equivalent of chlorine. If this view be applied to the sulphate of chloride of sulphur, it would also be a sulphuric acid, in which half an atom of oxygen is replaced by half a double atom of chlorine. In this case the compound would contain $\frac{1}{2}$ a volume of sulphur, 1 volume of chlorine, and $2\frac{1}{2}$ volumes of oxygen, and the calculated density of its vapour would be 7·414. In this case a very peculiar condensation must be supposed to occur, and yet the numbers would not agree with the experiment; from which it must be concluded, that the sulphate of chloride of sulphur, as it has hitherto been termed, is a compound of sulphate and of a chloride of sulphur which has not yet been isolated.

The sulphate of chloride of sulphur may be prepared by a much more simple process than that hitherto employed; it is effected by mixing chloride of sulphur with good Nordhausen sulphuric acid, and submitting the mixture to distillation with a gentle heat. It is thus obtained, mixed with hydrated sulphuric acid, from which it may be separated by a fresh distillation.

When a large quantity of anhydrous sulphuric acid is conveyed into a small quantity of chloride of sulphur, a blue compound is obtained. As it is very probable that chloride of sulphur is a solution of sulphur in a chloride of sulphur $S\,\overline{Cl}^2$, not yet isolated, the blue colour may be derived from the combination of this dissolved sulphur, it being well known that sulphur gives a blue solution with an excess of anhydrous sulphuric acid.—*L'Institut, Août* 1839.

ARSENIURETTED AND SUBARSENIURETTED CHLORIDE OF MERCURY.

M. Capitaine prepares these compounds by mixing three parts of calomel and one part of arsenic and heating the mixture in a matrass, on a sand heat. In the bottom of the vessel a reddish yellow hard mass is formed, which is not volatile, and which contains globules of mercury.

In the upper part of the matrass there is found a hard compact substance, sometimes yellow or reddish yellow, and on this occur dendritic crystals, which are small, opaque, of a fawn colour with a greyish tint; or larger crystals of a hyacinthine colour, which are sometimes short and curvilinear, and sometimes elongated and covered with crystalline concretions, the form of which is either the perfect tetrahedron, or the tetrahedron truncated on the solid angles.

The compact substance is variable in its colour, and is of uncertain composition. The yellowish dendritic crystals were found to consist of

Chlorine	11·76
Mercury	64·11
Arsenic	23·50——99·37

M. Capitaine considers it as composed of two equivalents of each of its elements; reckoning, however, chlorine, mercury, and arsenic respectively as 36, 202 and 38, it will appear to be a compound of

1 eq. of Chlorine	36
1 eq. of Mercury	202
2 eqs. of Arsenic	76

This substance is very readily acted upon by light, especially when powdered; when exposed to the action of the solar rays it becomes in a few seconds of a greenish colour, and passing gradually through deeper tints, it becomes eventually quite black. Neither the air nor the water which the crystals contain has any effect in producing these phænomena; for when exposed to light in a dry vacuum, it becomes brown as rapidly as in the air. The effects in both cases are produced more readily by the direct rays of the sun, than in a diffused light.

The crystals of a hyacinthine red colour are not so constant in their composition as the foregoing, and the results of analysis were not so satisfactory. The quantity of chlorine varied from 12 to 14 per cent., and that of the mercury varied from 68, 70, 71, 73, and even 75 per cent., the remainder being arsenic.

The compound was probably composed of

1 eq. of Chlorine	36
1 eq. of Mercury	202
1 eq. of Arsenic	38——276

If this be the case, the first compound, called by M. Capitaine arseniuretted chloride of mercury, is bi-arseniuretted chloride, and this which he terms sub-arseniuretted, is in fact the arseniuretted. These compounds resemble each other in the decomposition which they both undergo by the action of water and of heat. When treated with water, especially at a boiling heat, they are completely decomposed into mercury, arsenic, arsenious and hydrochloric acids. When acted upon by heat, a portion is volatilized without undergoing any alteration; while another portion is decomposed yielding mercury, arsenic, and chloride of arsenic.—*Journal de Pharmacie*, September, 1839.

ANALYSIS OF ALBUMEN.

In order to verify the late analysis of albumen by M. Mulder, M. Vogel employed the white of hens' eggs coagulated by heat, because in this state the foreign salts which it contains are more readily removed by washing with water than when in the liquid state.

The white of egg hardened by ebullition was carefully separated from the exterior pellicle and from every adhering portion of yelk; it was then cut into pieces and washed for several days with distilled water, till it ceased to dissolve anything. It was then dried between folds of blotting-paper at the ordinary temperature of the room; after some days it became hard, brittle, of a pale yellow, translucent and

almost transparent, and had in fact the appearance of gum arabic; it was gritty between the teeth, and softened but slowly in water; it had lost 90 per cent. of its weight in drying. It was then reduced to fine powder and again dried at the temperature of 212°; this drying was repeated previously to every analysis, in order to get rid of the hygrometric moisture.

The white of egg in this state was repeatedly digested in æther to remove the fatty matter.

To determine the amount of inorganic matter, a portion of the dry substance was incinerated in a platina crucible; the residue amounted to exactly $2\frac{1}{3}$ per cent.; it consisted principally of phosphate of lime and a little sulphate of lime. Berzelius found 1·8 per cent., and Mulder 4·12 per cent.,—the proportion does not appear therefore to be always equal.

M. Vogel recommends those who would repeat these experiments, not to incinerate in a platina crucible, on account of the action which the phosphorus separated from the acid may have on the metal. He recommends a porcelain crucible, and the use of a little nitric acid or nitrate of ammonia to expedite the process.

The white of egg, prepared as has been described, was analysed by oxide of copper and chromate of lead, while a third analysis was performed on a portion which had been dissolved in potash and precipitated by sulphuric acid. The results were

	I.	II.	Precipitated Albumen. III.
Carbon	51·228	52·817	51·856
Hydrogen	7·232	7·206	6·766
Azote	16·465	16·970	16·403
Oxygen	22·745	20·677	22·645 ?
Ashes	2·330	2·330	2·330 ?
	100·	100·	100·

These results, excepting the azote, agree very nearly with those obtained by M. Mulder, viz.

Carbon	52·43	
Hydrogen	6·73	
Azote	15·30	
Oxygen	21·12	
Ashes	4·12	——99·7

M. Vogel obtained, as M. Mulder had done, a greenish blue salt of copper, by treating a solution of white of egg in distilled water with one of sulphate of copper; and he considers it also as an albumenate of copper; and he ascertained by analysis that the albumen undergoes no alteration in this combination.—*Journal de Pharmacie*, September, 1839.

ANALYSIS OF FIBRIN.

M. Vogel obtained fibrin from ox blood. The blood was stirred; the small filaments of fibrin were washed with water till it came away colourless; when thus purified the fibrin has a bluish white colour like skimmed milk. It was dried first at the temperature of

the air, between folds of blotting-paper, then at 212°, reduced to fine powder, repeatedly treated with æther, and then again dried.

By being burnt in a porcelain crucible, with the addition of nitric acid, it left 2·66 per cent. of ashes; these consisted of phosphate and sulphate of lime, with a rather considerable proportion of oxide of iron.

Analysed by means of chromate of lead, it yielded

Carbon	52·406
Hydrogen	7·094
Azote	18·120
Oxygen	19·720
Ashes	2·660
	100·

The combinations of fibrin and the products of its decomposition are as yet but little known. If recently coagulated albumen be boiled in water in a Papin's digester at a temperature somewhat at or about 212°, it dissolves almost entirely after some hours. This solution is not precipitated by alcohol, but is so by a solution of alum, protonitrate of mercury, tannin, and hydrochloric acid; neither the acetate nor subacetate of lead produces this effect. The solution contains no gelatin; for even when it is very concentrated it does not gelatinize on cooling. When evaporated to dryness, it leaves a brittle, transparent substance resembling gum, which redissolves in hot water. When boiled with an excess of hydrochloric acid, it assumes the same fine lilac colour as fresh fibrin similarly treated.

If fibrin be boiled with dilute sulphuric acid, and peroxide of manganese be added to the boiling liquor, decomposition occurs; a peculiar penetrating odour is developed, in which that of formic acid is perceptible. There is a solution of organic matter in the liquor, which may be obtained in a pure state by means of carbonate of barytes or of lime, and evaporating the filtered liquor. It has not been more particularly examined. Fibrin is also altered by chlorine; when long exposed to an aqueous solution of it, the greater part is dissolved; but a white powder is gradually precipitated to the bottom, probably because the liquid chlorine is gradually decomposed. —*Journal de Pharmacie*, September, 1839.

NOTICE FROM DR. ROBERT HARE, PROFESSOR OF CHEMISTRY, ECT., RESPECTING THE FUSION OF PLATINA, ALSO RESPECTING A NEW ÆTHER, AND A SERIES OF GASEOUS COMPOUNDS FORMED WITH THE ELEMENTS OF WATER.

TO PROF. SILLIMAN.

Philadelphia, Dec. 15th, 1838.

My dear Friend,—I send you for the Journal a brief notice of some results, observations, and inferences, which are nearly in the same language in which they were communicated to the Chemical Section of the British Association for the Advancement of Science.

I have by improvements in my process for fusing platina, suc-

ceeded in reducing twenty-five ounces* of that metal to a state so liquid, that the containing cavity not being sufficiently capacious, about two ounces overflowed it, leaving a mass of twenty-three ounces. I repeat that I see no difficulty in extending the power of my apparatus to the fusion of much larger masses.

When nitric acid or sulphuric acid with a nitrate is employed to generate æther, there must be an excess of two atoms of oxygen for each atom of the hyponitrous acid which enters into combination. This excess involves not only the consumption of a large proportion of alcohol, but also gives rise to several acids and to some volatile and acrid liquids.

It occurred to me that for the production of pure hyponitrous æther a hyponitrite should be used. The result has fully realized my expectations.

By subjecting hyponitrite potassa or soda to alcohol and diluted sulphuric acid, I have obtained a species of æther which differs from that usually known as nitrous or nitric æther in being sweeter to the taste, more bland to the smell, and more volatile. It boils below 65° of F., and produces by its spontaneous evaporation a temperature of 0 − 15° F. On contact with the finger or tongue it hisses as water does with red-hot iron. After being made to boil, if allowed to stand for some time at a temperature below its boiling point, ebullition may be renewed in it apparently at a temperature lower than that at which it had ceased. Possibly this apparent ebullition arises from the partial resolution of the liquid into an aeriform æthereal fluid, which escapes, both during the distillation of the liquid æther and after it has ceased, at a temperature below freezing. This aeriform product has been found partially condensible, by pressure, into a yellow liquid, the vapour of which, when allowed to enter the mouth or nose, produced an impression like that of the liquid æther. I conjecture that it consists of nitric oxide, so united to a portion of the æther as to prevent the wonted reaction of this gas with atmospheric oxygen. Hence it does not produce red fumes on being mingled with air.

Towards the end of the ordinary process for the evolution of the sweet spirits of nitre, a volatile acrid liquid is created which affects the eyes and nose like mustard, or horse radish.

When the new æther as it first condenses is distilled from quicklime, this earth becomes imbued with an essential oil which it yields to hydric æther. This oil may be afterwards isolated by the spontaneous evaporation of its solvent. It has a mixed odour, partly agreeable, partly unpleasant. From the affinity of its odour and that of common nitrous æther, I infer that it is one of the impurities which exist in that compound.

The new æther is obtained in the highest degree of purity, though in less quantity, by introducing the materials into a strong well-ground stoppered bottle, refrigerated by snow and salt. After some time the æther will form a supernatant stratum, which may be sepa-

* Troy weight. The actual quantity fused was 12,250 grs.; the lump remaining weighed 10,937 grs.

rated by decomposition. Any acid, having a stronger affinity for the alkaline base than the hyponitrous acid, will answer to generate this æther. Acetic acid not only extricates but appears to combine with it, forming apparently a hyponitro-acetic æther.

I observed some years ago that when olefiant gas is inflamed with an inadequate supply of oxygen, carbon is deposited, while the resulting gas occupies double the space of the mixture before explosion. Of this I conceive I have discovered the explanation. By a great number of experiments, performed with the aid of my barometer-gauge eudiometer, I have ascertained that if during the explosion of the gaseous elements of water any gaseous or volatile inflammable matter be present, instead of condensing there will be a permanent gas formed by the union of the nascent water with the inflammable matter. Thus two volumes of oxygen, with four of hydrogen, and one of olefiant gas, give six volumes of permanent gas, which burns and smells like light carburetted hydrogen. The same quantity of the pure hydrogen and oxygen with half a volume of hydric æther gives on the average the same residue. One volume of the new hyponitrous æther under like circumstances produced five volumes of gas.

An analogous product is obtained when the same aqueous elements are inflamed in the presence of an essential oil. With oil of turpentine a gas was obtained weighing per hundred cubic inches $16\frac{5}{10}$ grs., which is nearly the gravity of light carburetted hydrogen. The gas obtained from olefiant gas, or from æther, weighed on the average, per the same bulk $13\frac{5}{10}$ grs. The olefiant gas which I used weighed per hundred cubic inches only $30\frac{4}{10}$ grs. Of course if *per se* expanded into six volumes, it could have weighed only one sixth of that weight, or little over five grains per hundred cubic inches. There can therefore be no doubt that the gas obtained by the means in question, is chiefly constituted of water, or of its elements in the same proportion H^2O.

With a volume of the new æther, six volumes of the mixture of hydrogen and oxygen give on the average about five residual volumes. The gas created in either of the modes above mentioned does not contain carbonic acid, and when generated from olefiant gas appears by analysis to yield the same quantity of carbon and hydrogen as that gas affords before expansion.

These facts point out a source of error in experiments, for analysing gaseous mixtures by ignition with oxygen or hydrogen, in which the consequent condensation is appealed to as a basis for an estimate. It appears that the resulting water may form new products with certain volatilizable substances which may be present.

From the account of the proceedings of the Section, published in the Athenæum, it appears, that after my letter, in which the facts above mentioned were stated, was read, a Mr. Maugham, who is employed to exhibit the hydro-oxygen microscope at the Adelaide Gallery, London, asserted that I had accomplished the fusion, of which mention has been above made, by means of a blowpipe of his contrivance, which I had purchased while in London.

The opinion which I am obliged to entertain of an individual capable of this groundless assertion, would cause me to consider him unworthy of notice, had not his misstatement been made before an assemblage which I most highly esteem, and had he not been honoured by a premium for his pretended invention by a respectable British Society.

The blowpipe which is thus falsely alleged to have been used by me, differs immaterially from one of which I published an engraving and description in the American Journal of Science for 1820, vol. ii. p. 298, being a modification of that originally contrived by me and republished in Tilloch's Philosophical Magazine, vol. xiv. for 1802.

Between the instruments described in these publications, or in the Franklin Journal, and that employed by Maugham, the only difference worthy of notice is, that the latter is near the apex bent so as to form an acute angle, and is thus rendered suitable for directing the flame upon a revolving cylinder of lime.

Although I purchased of Newman a blowpipe bent as described, with an apparatus attached for holding and turning a cylinder of lime, *I have never made any use of it,* having for the purpose of subjecting lime to the flame, found my modification above referred to as described in this Journal, preferable. It only required the jet pipe to be directed upwards in an angle of about forty-five degrees with the axis of the lime cylinder.

I do not consider the form of my blowpipe employed by Mr. M. as qualified for the fusion of any metal.

It is remarkable that an apparatus of gasometers employed by Maugham at the Adelaide Gallery for the supply of the gases for the blowpipe differs but little from the apparatus proposed for the same purpose in my communication above adverted to, and published nearly twenty years ago.

However the process by which I have lately extended the power of the hydro-oxygen blowpipe may differ from those to which I had previously resorted, it differs still more from that modification which Maugham has claimed as his own.—*American Journal of Science and Arts, vol.* 35, *No.* 2.

ACTION OF FERROCYANIDE OF POTASSIUM ON CHLORIDE OF CALCIUM, ETC.

M. Reiset added a solution of ferrocyanide of potassium to one of chloride of calcium: the well-known white, crystalline and slightly soluble precipitate was obtained, which he found to consist of one equivalent of ferrocyanide of potassium and one equivalent of ferrocyanide of calcium. He further observed that compounds of analogous constitution are formed when the salts barium, strontium, or magnesium are substituted for those of calcium.

The crystals obtained when a solution of chloride of barium is poured into a hot solution of ferrocyanide of potassium, and which are described in many chemical treatises as being pure ferrocyanide of barium, are composed of 1 equivalent of ferrocyanide of potassium and 1 equivalent of chloride of barium. The ferrocyanide of ammo-

nia also possesses the property of combining atom to atom with the earthy ferrocyanides.—*L'Institut, August,* 1839.

PURPURIC ACID.

M. Fritzche in January last, read before the Academy of Sciences of St. Petersburgh a notice respecting purpuric acid and its salts.

It is not long since that the name of purpuric acid was given to a substance discovered by Prout, which had neither a purple colour, nor possessed acid properties, but it was so named, because it was extracted, by means of an acid, from purpurate of ammonia*. The researches of Liebig and Wöhler on the products of the decomposition of uric acid by nitric acid, have proved that this substance is the product of the complete decomposition which purpurate of ammonia undergoes by most acids, and they have consequently given it the very proper name of *murexane*; but this is not the case with the new name of *murexide*, which these chemists have applied to purpurate of ammonia on account of its not being a salt but an *amide*. This principle could be correct if only one ammoniacal body could be thus formed; but in the year 1818, Prout had shown that a great number of other purpurates could be obtained with the purpurate of ammonia; and soon afterwards Vauquelin described the properties of purpurate of silver: more lately Kodweis analysed the purpurate of barytes; and from this period the purpurates are described in all treatises on chemistry, although the composition of purpuric acid is unknown, and it was known only that it transformed ammoniacal salts into another class of substances. The memoir of M. Fritzche has for its object the reestablishment of the name of purpuric acid, to prove that it really exists as an acid in the salts which have been called purpurates, which is known only in combination with bases, to analyse the purpurates, and to supply the deficiencies which still exist as to our knowledge of the compounds which this acid may form. The following are the results obtained by M. Fritzche.

Purpuric acid has not yet been isolated; when attempts are made to separate it from its combinations by means of acids, it is decomposed, and yields, in dilute solutions, principally *murexane*, whereas with concentrated solutions it forms other products. Purpuric acid forms with bases, salts which are slightly soluble, and remarkable for the fine purple colour of their solutions. Besides these neutral salts, in which the base is to the acid as 1 to 10, it forms basic salts also, but not acid salts. When perfectly free from water, purpuric acid consists of

Hydrogen......	1·581	$= H^{8}$
Carbon........	38·725	$= C^{16}$
Oxygen	31·665	$= O^{10}$
Azote	28·029	$= N^{10}$
	100·	

* This is not correctly stated. Although purpurate of ammonia had been long known, it did not receive that name until after Dr. Prout had obtained the acid from it, which, at the suggestion of Dr. Wollaston, from its forming purple salts, he then called *purpuric* acid. See Phil. Trans. for 1818, or Phil. Mag., First Series, vol. liii. p. 25.—Edit.

PURPURATE OF SILVER.

This salt, first obtained by Prout, was dried at 266° Fahr. It was analysed by the usual methods, and gave by

	Experiment.	Calculation.	
Hydrogen	1·31	1·32	or H^{10}
Carbon	25·75	25·89	C^{16}
Oxygen	23·19	23·30	O^{11}
Azote	19·02	18·75	N^{10}
Oxide of silver ..	30·73	30·74	$\dot{A}g$
	100·	100·	

The basic purpurate of silver is obtained by adding ammonia to nitrate of silver until the precipitate is redissolved, and with this solution purpurate of ammonia is to be precipitated. The author could not effect the analysis of this salt, for having exposed it to a temperature of 392° Fahr. in an oil-bath, the whole mass was decomposed.

PURPURATE OF POTASH.

This salt was obtained by Prout, who recommends it to be prepared by decomposing a boiling solution of purpurate of ammonia by means of a solution of bicarbonate of potash; but as it is in this method impossible to avoid a disengagement of ammonia, which when heated acts upon the purpurate, it is better to employ nitrate of potash. The purpurate of potash is difficultly soluble in water, but not insoluble; it is much less soluble in saline solutions: on this account it is proper to employ great excess of nitre in preparing it. This purpurate is pulverulent, and consists of very small reddish brown microscopic crystals; it may also be obtained in large crystals, which have the colour and lustre of the ammoniacal compound, or perhaps rather deeper. The analysis of this salt, carefully performed, gave

	By Experiment.	Calculation.	
Hydrogen......	1·33	1·33	or H^8
Carbon........	31·23	32·63	C^{16}
Azote	24·05	23·62	N^{10}
Potash	15·48	15·73	$\dot{K}$
		26·69	O^{10}
		100·	

The indigo blue liquid which is obtained when purpurate of ammonia is dissolved in a solution of caustic potash, is probably derived from the formation of a basic salt, which cannot be procured in a solid form.

PURPURATE OF AMMONIA.

The properties of this salt are perfectly well known by the experiments of MM. Liebig and Wöhler, but still neither the composition nor the formula which they have given is correct. In order to procure this salt, which is prepared with difficulty by the usual methods, M. Fritzche employed a process which yields it readily and in great abundance. This process is derived from the observation he had

made, that a large quantity of purpurate of ammonia may be formed by the action of ammonia on pure alloxane; when a concentrated and boiling solution of alloxane is gradually added to a solution of carbonate of ammonia, carbonic acid is disengaged with strong effervescence; the solution on each addition assumes a deeper purple tint, and at last it becomes turbid and precipitates a great quantity of reddish powder, which consists of pure anhydrous purpurate of ammonia. Instead of using pure alloxane, concentrated but not fuming nitrate of uric acid may be employed, taking care to adopt the precautions given by Liebig and Wöhler for the preparation of alloxane. Purpurate of ammonia is not the only product of the operation, but as the composition of the others produced is not known, it would be difficult to give a plausible explanation of the formation of this salt. The analysis of purpurate of ammonia dried at 212° Fahr. yielded M. Fritzche results which differed from those obtained by MM. Liebig and Wöhler. These successive experiments gave

Hydrogen	2·82	2·84	2·83
Carbonic acid	34·78	34·43	35·52
Azote	30·70	30·89	

The mean of these analyses agrees very well with the following view:

	Experiment.	Calculation.	
Hydrogen......	2·83	2·86	or H^{16}
Carbon	34·91	35·10	C^{16}
Oxygen	31·47	31·56	O^{11}
Azote	30·79	30·48	N^{12}
	100·	100·	

The crystals of purpurate of ammonia contain water of crystallization, amounting according to Liebig and Wöhler to 3 or 4 per cent., whereas M. Fritzche found as the mean of eight experiments on very pure crystals 6·06 per cent.

PURPURATE OF BARYTES.

This salt is obtained by mixing a solution of barytes with a moderately concentrated solution of purpurate of ammonia; the liquor becomes turbid, and a blackish green powder is deposited, which by friction becomes of deep purplish red. M. Kodweis has given an analysis of this salt to fix the atomic weight of murexane, but the result shows, that he had not sufficiently dried it: taking care to do this, M. Fritzche obtained by

	Experiment.	Calculation.	
Hydrogen	1·717	1·725	or H^{12}
Carbon	27·980	28·180	C^{16}
Barytes	21·960	22·045	$\dot{Ba}$
Oxygen......		27·650	O^{12}
Azote		29·100	N^{10}
		100·*	

* There are some errors in this statement: the sum of the quantities stated is not 100.—Ed.

So that this salt dried at 212° would contain 22·045 of barytes 72·772 of purpuric acid, and 5·83 of water; and dried at common temperatures, barytes 19·98, purpuric acid 65·94 and water 14·08. —*L'Institut*, No. 304.

ACTION OF ANTIMONY ON BICHLORIDE OF MERCURY.

M. Capitaine has examined the action of these substances on each other. When one part of commercial antimony and three parts of bichloride of mercury are heated together in a retort with its proper appendages, the first portions of chloride of antimony which distil are of a reddish brown colour; and the adopter is lined with the same substance which discolours the first products; soon afterwards the chloride of antimony distils colourless and perfectly pure. When the operation is finished, there is found in the retort on the revived mercury, a blackish substance; if this be heated, it yields mercury, a little butter of antimony, and towards the end it yields reddish yellow vapours, which condense into a solid of the same colour, and which it is impossible not to recognise as arseniuretted chloride of mercury. It has the appearance and all the chemical characters of this compound: when treated with boiling water, it yields a liquor containing hydrochloric and arsenious acid, and leaves a black residue, which put on a red-hot coal is volatilized, emitting an alliaceous smell.

If the chloride of antimony obtained be kept in fusion for some time, all the brown matter which discolours it is deposited and readily separated. It contains metallic mercury, and also the compound of protochloride of mercury and of arsenic.

The chloride of antimony may be obtained by decantation, but it is better to redistil it; the small portion of protochloride of mercury which it may contain, attaches itself to the neck of the retort. It is then entirely free from arsenic; the antimony extracted from it yields completely inodorous vapours.

These experiments show, what was previously known, that with antimony containing arsenic, chloride of antimony may be obtained quite pure; but they also show what becomes of the arsenic; they prove that, combined with the protochloride of mercury, it remains in the distilling apparatus, and that any portion of it which the chloride of antimony may contain may be separated by rest or redistillation.

It is to be observed, as a very important circumstance, that this facility of preparing chloride of antimony with arsenial antimony happens only when the proportions of antimony and bichloride of mercury mentioned in different authors are employed. These are one of antimony and three of the bichloride; they are equivalent to one atom of antimony, to a little less than one and a half of bichloride; there is consequently an excess of antimony: if an excess of bichloride were used, the first product would be white, but would contain chloride of arsenic.

When antimony free from arsenic is made to act upon bichloride of mercury, the first product is not of a reddish brown colour, but merely greyish, on account of a little very finely divided mercury which passes over with it.—*Journal de Pharmacie*, Sept. 1839.

BLOOD CORPUSCLES IN THE MAMMALIA.

We hope soon to publish an account of Mr. Gulliver's observations (in which he has been long engaged) on the blood disks of the mammalia. In the meantime we may mention that in five Australasian animals the corpuscles have the form and size most common in mammals, their diameters varying from $\frac{1}{4800}$th to $\frac{1}{3000}$th of an inch. These Australasian animals are the *Perameles lagotis, Petaurus Sciurus, Macropus Bennettii, Dasyurus Ursinus,* and *D. Viverrinus.*

In reference to the interesting discovery by M. Mandl of the oval blood corpuscles of the Dromedary, Mr. Gulliver has found the blood-disks of the *Auchenia Vicugna, A. Paco,* and *A. Glama,* also very distinctly elliptical. In the *Vicugna* they are rather smaller than in the other species.

In the Musk Deer (*Tragalus Javanicus*) Mr. Gulliver observes that the blood disks are smaller than those, hitherto described, of any other mammal whatever. In the *Tragalus,* the disks, though very distinct in form, measure on an average $\frac{1}{12000}$th of an inch only; but many variations in size are to be seen, from $\frac{1}{15000}$ th to $\frac{1}{9000}$th of an inch in diameter.

METEOROLOGICAL OBSERVATIONS FOR OCT., 1839.

Chiswick.—Oct. 1. Foggy: very fine. 2. Foggy: rain. 3. Very fine. 4. Rain: stormy with rain at night. 5. Boisterous: clear. 6. Fine. 7. Cloudy: fine. 8. Hazy: very fine. 9. Very fine: heavy rain at night. 10. Showery: sultry at intervals. 11. Very fine. 12. Fine: rain at night. 13. Foggy: fine. 14. Fine. 15, 16. Slight fog: fine. 17. Foggy. 18. Drizzly. 19, 20. Foggy: fine. 21, 22. Hazy: fine. 23, 24. Rain. 25. Overcast: fine. 26, 27. Clear and fine. 28. Stormy: showers of rain. 29, 30. Hazy and cold. 31. Rain.

Boston.—Oct. 1. Fine. 2. Cloudy: rain P.M. 3. Fine. 4. Rain: rain early A.M. 5. Cloudy: rain early A.M. 6. Cloudy. 7, 8. Fine. 9. Cloudy. 10. Rain. 11. Fine. 12. Cloudy. 13. Cloudy: rain early A.M. 14, 15. Cloudy. 16. Fine. 17. Cloudy. 18. Cloudy: rain A.M. 19. Fine. 20, 21. Foggy. 22, 23. Cloudy. 24. Rain. 25. Cloudy. 26. Fine: rain early A.M. 27. Fine: rain P.M. 28. Rain: rain early A.M. 29. Cloudy: rain: stormy night. 30. Stormy. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Wet throughout. 2. Very wet morning: cleared at noon. 3. Fair till 11 A.M., when began to rain. 4. Fair all day. 5. Fine calm day: hoar frost morning and ice. 6. The same: hoar frost still stronger. 7. A very good harvest day: getting cloudy P.M. 8. Drizzling all day. 9. The same. 10. The same A.M.: fair P.M. 11. Fair throughout. 12. The same. 13. The same: overcast in the evening. 14. Drizzling all day. 15. Dull, cloudy, and damp: rain P.M. 16. Sunshine and showers alternately. 17. Very fine day: hoar frost early A.M. 18, 19. The same: ice on the ponds. 20. Very fine day. 21. Fair; but dull and foggy. 22. Fair till noon, when rain came on. 23. Rain all day. 24. Rain all day though slight. 25, 26. Fair throughout. 27. Clear and temperate. 28. Fine October day. 29. One slight shower, when it cleared. 30. Fair: air very keen. 31. The same: keen and cold like a day in March.

Sun 24 days. Rain 13 days. Hoar frost 4 days.

Wind north $1\frac{1}{2}$ day. North-west $\frac{1}{2}$ day. North-east 1 day. East 5 days. East-north-east 8 days. South 4 days. South-east 4 days. South-south-east 1 day. West 1 day. South-west 3 days. West-south-west 1 day. Variable 1 day.

Calm 8 days. Moderate 11 days. Brisk 7 days. Strong breeze 4 days. Boisterous 1 day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1839. Oct.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·000	29·988	29·856	29·55	29·79	29·59	53·4	58·4	48·4	68	41	51	54	42	ENE.	SE.	calm	SW.	...	...	...	...	51
2.	29·688	29·803	29·659	29·20	29·39	29·56	55·8	61·8	50·9	66	37	52	54	45½	S.	W.	calm	W.	...	·12	...	...	51
3.	30·004	29·992	29·790	29·44	29·70	29·53	48·7	63·0	44·2	64	48	47·5	53	39	SSW.	S.	calm	SW.	·102	·21	·10	...	44
4.	29·456	29·655	29·447	28·94	29·50	29·83	54·8	55·3	47·9	65	49	53	50	43	S.	S.	calm	N.	·172	·27	·42	...	51
5.	30·018	30·277	29·978	29·60	30·07	30·16	49·5	60·0	49·3	55	41	50	51	34	NW.	N.	N.	N&NW	·538	...	·47	...	49
● 6.	30·302	30·351	30·302	29·82	30·22	30·19	49·8	52·3	44·7	58	38	51	50	29	N.	N.	calm	Var.	...	...	...	1·23	47
7.	30·298	30·315	30·226	29·73	30·13	29·97	51·3	52·0	45·9	62	43	51	54	38	N.	NE.	calm	SW.	...	...	...	·	46
8.	30·120	30·130	29·861	29·52	29·83	29·73	52·8	53·3	51·0	66	56	49	52½	48½	NW.	SE.	calm	S.	...	...	...	..	47
9.	29·876	29·800	29·690	29·24	29·57	29·49	59·7	60·0	51·4	68	56	58	54	48½	S.	SW.	calm	S.	...	·23	...	...	55
10.	29·642	29·687	29·618	29·02	29·29	29·35	60·6	60·8	58·2	68	48	58	57	49	S.	S.	W.	SSE.	·027	·07	·11	...	57
11.	29·598	29·651	29·495	29·02	29·44	29·58	59·7	62·3	54·7	71	52	59	61	50	E.	S.	NE.	E.	·036	·31	·03	...	57
12.	29·844	30·050	29·788	29·23	29·63	29·85	56·3	63·5	54·3	60	42	58·5	60	51	S.	S.	S.	E.	·250	·01	...	..	53
13.	30·020	30·016	29·987	29·38	29·84	29·70	52·3	52·4	49·6	65	37	55	57	44	SE.	W.	calm	E.	...	...	·09	...	51
☽ 14.	29·938	29·920	29·815	29·44	29·59	29·55	54·8	55·0	48·0	61	42	52	57	50	SE.	S.	calm	E.	...	·02	...	1·27	52
15.	29·818	29·937	29·833	29·30	29·67	29·68	52·2	59·0	50·2	59	38	52	55	47½	S.	...	calm	SE.	...	...	...	...	49
16.	30·074	30·066	30·037	29·47	29·66	29·72	52·7	52·5	46·2	61	36	47	54½	44½	SW.	S.	W.	S.	·033	...	...	...	48
17.	30·092	30·085	30·050	29·61	29·98	30·00	48·0	51·3	43·8	57	48	46	54	40½	S.	S.	calm	E.	...	·14	...	...	46
18.	29·866	29·917	29·861	29·30	29·88	29·82	50·8	54·7	47·5	59	45	51	52	35	E.	S.	calm	WSW.	·219	·06	...	...	50
19.	30·044	30·133	30·033	29·53	29·89	29·91	50·2	51·6	47·0	61	35	45	50	30½	SW.	SW.	calm	SE.	·019	...	·07	...	48
20.	30·092	30·097	30·036	29·62	29·96	29·90	41·2	55·6	49·6	60	44	45	51½	35	W.	SE.	calm	NE.	...	...	...	...	43
○ 21.	30·064	30·075	30·048	29·55	29·89	29·85	54·0	54·3	41·0	59	51	52	52½	36½	ENE.	NE.	calm	S.	...	...	...	2·30	49
22.	30·156	30·148	30·106	29·59	29·80	29·83	53·3	53·6	52·8	56	51	52	54	45	SE.	S.	calm	SE.	...	·01	...	...	50
23.	30·086	30·090	30·062	29·54	29·80	29·80	53·2	53·4	52·7	54	49	53	54½	48	SE.	SE.	calm	SE.	...	·24	...	...	49
24.	30·050	30·131	30·032	29·04	29·87	30·08	48·8	49·5	49·3	54	44	50	53½	50	E.	S.	S.	ENE.	·175	·30	·15	...	49
25.	30·264	30·358	30·252	29·76	30·27	30·35	48·3	50·0	46·4	52	41	49	54	45	NNE.	E.	calm	ENE.	·266	...	·28	...	47
26.	30·330	30·338	30·208	29·88	30·35	30·31	46·3	50·4	44·0	53	36	44	48½	39½	N.	NE.	calm	ENE.	...	...	·01	...	45
☾ 27.	30·272	30·296	30·247	29·78	30·28	30·32	42·0	50·0	40·6	51	39	43	57	37½	NW.	NE.	calm	ENE.	...	·02	...	...	42
28.	30·314	30·480	30·322	29·91	30·41	30·47	42·7	49·0	40·4	46	37	44	48½	36	N.	NW.	NE.	ENE.	...	...	·26	0·05	39
29.	30·316	30·446	30·222	29·96	30·43	30·40	42·7	47·2	37·3	47	39	44·5	48½	36	NE.	NE.	NE.	ENE.	·060	·12	·42	...	37
30.	30·142	30·161	30·038	29·81	30·30	30·22	41·4	45·0	39·8	48	38	47	49	38½	NE.	NE.	E.	ENE.	...	...	...	...	38
31.	29·940	29·974	29·853	29·60	30·11	30·02	39·3	44·8	39·8	42	38	43	47½	43	NE.	E.	E.	ENE.	·061	·10	...	...	38
Mean.	30·023	30·076	29·959	29·49	29·885	29·895	50·5	54·3	47·3	58·58	44·23	50·	53·2	41·9					Sum.	2·23	2·41	4·85	Mean.

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LXXV. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions. By* JAMES IVORY, *K.H., M.A., F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

[Continued from p. 395.]

10. THE equation (C.) supposes that the atmosphere consists entirely of dry air: we have next to consider what modification must be made when it contains a portion of aqueous vapour.

In the first place, when p' and τ', the pressure and temperature at the surface of the earth, are given, as they are in the mean atmosphere which produces the refractions, the quantity α, or the refractive power of the air, is not liable to be altered by any possible mixture of aqueous vapour. For if an addition of vapour to dry air diminish the refractive power by making the density less, the greater action of the vapour upon light is found almost exactly to compensate the defect. Laplace first made this observation; which has been confirmed by MM. Biot and Arago, who have established by experiments, that the refractive power of air, whether dry or mixed with vapour, is the same, when the pressure and temperature are the same. It thus appears that, as far as the quantity α, or the refractive power of the air at the earth's surface, is concerned, the astronomical refractions are independent on the hygrometric condition of the atmosphere.

But a mixture of vapour may produce changes in the expression of the refraction, by altering the coefficients or the integrals. Now, if we attend to the formulas that have been found for an atmosphere of moist air, and in the equation (10.) make the same substitution as in the case of dry air, viz.

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha\,\omega,$$

we shall obtain

$$s \,.\, \frac{(\rho')}{p'} = \frac{s\left(1 - \frac{3}{8}\,\frac{\phi'}{p'}\right)}{L\,(1+\beta\,\tau')} = x,$$

$$\frac{p'}{(\rho')} \cdot \frac{1}{a} = \frac{L(1+\beta\tau')}{a\left(1-\frac{3}{8}\cdot\frac{\phi'}{p'}\right)} = i,$$

$$\alpha \cdot \frac{a(\rho')}{p'} = \frac{\alpha}{i} = \frac{a\left(1-\frac{3}{8}\cdot\frac{\phi'}{p'}\right)}{L(1+\beta\tau')} = \lambda:$$

and further, it will appear that the same relation subsists between x and u in the atmosphere of moist air as between the quantities represented by the same letters in the atmosphere of dry air. The same procedure will therefore lead, in both cases, to the same integrals extending between the same limits. The only difference lies in the values of λ and i, which in the case of moist air acquire, as a multiplier or divisor, the small factor $\left(1-\frac{3}{8}\cdot\frac{\phi'}{p'}\right)$ depending on the tension of the vapour at the earth's surface. If the hygrometer afforded an easy practical method of ascertaining the tension of the vapour, the minute variations of the refractions, arising from moisture in the atmosphere, might be corrected by the method usually employed for compensating the small changes which a difference of temperature causes in the mean constants.

Experience confirms what has been said; for all the astronomers who have attended to aqueous vapour in the atmosphere, agree in admitting that it either has no influence, or but a very small and imperceptible effect, to alter the refractions. On this head it will be sufficient to cite the authority of M. Biot*, who seems carefully to have studied this point, on which he expresses himself very strongly. The very exact coincidence of the theoretical with the observed refractions as far as 88° or $88\frac{1}{2}$° from the zenith, concurs to prove that the variable quantity of vapour in the air has little influence so long as it retains the gaseous form; but at lower altitudes, when the rays of light become almost parallel to the horizon, it is very probable that particular and local causes may come into play.

11. Nothing is now wanted for completing the solution of the problem, except the reducing of the expression (C.) to a form fit for numerical calculation.

* *Précis Elem. de Physique*, p. 229, tom. ii. edit. 2nd. *Addit. à la Conn. des Temps.* 1839, p.. 36.

Investigation of the integral Q_0.

We have

$$Q_0 = \int_0^m \frac{e\,d\,x\,c^{-x}}{\Delta} = \int_0^m \frac{e\,d\,x\,c^{-x}}{\sqrt{(1-e^2)^2+4e^2.\frac{x}{m}}}:$$

assume,

$$\Delta = \sqrt{(1-e^2)^2+4e^2.\frac{x}{m}} = 1-e^2+2e^2z;$$

then,

$$\frac{e\,d\,x}{\Delta} = e.m\,d\,z,$$

$$\frac{x}{m} = t = 1 - e^2(z-z^2).$$

By Lagrange's theorem,

$$\Psi = t - t^2,$$

$$z = t + e^2.\Psi + \frac{e^4}{1.2}.\frac{d.\Psi^2}{d\,t} + \frac{e^6}{1.2.3}.\frac{d\,d.\Psi^3}{d\,t} + \&c.;$$

$$m\,dz = m\,dt.\left\{1+e^2.\frac{d.\Psi}{d\,t}+\frac{e^4}{1.2}.\frac{d^2\Psi^2}{d\,t^2}+\frac{e^6}{1.2.3}.\frac{d^3\Psi^3}{d\,t^3}+\&c.\right\};$$

consequently,

$$\int_0^m \frac{e\,d\,x\,c^{-x}}{\Delta} = \int_0^1 m\,d\,t\,c^{-m\,t}\left\{e+e^3\frac{d\Psi}{d\,t}+\frac{e^5}{1.2}.\frac{d^2\Psi^2}{d\,t^2}+\&c.\right\}.$$

Wherefore, if we assume

$$Q_0 = A_1 e + A_3 e^3 + A_5 e^5 + \&c.,$$

we shall have

$$A_{2n+1} = \frac{1}{1.2.3\ldots n}\int_0^1 m\,d\,t\,c^{-mt}.\frac{d^n.\Psi^n}{d\,t^n}.$$

In the first place, it may be proper to show that all the coefficients in the series for Q^0 are positive. For this purpose integrate by parts, and the results will be

$$A_{2n+1} = \frac{m}{1.2.3\ldots n}\times\left\{c^{-m\,t}.\frac{d^{n-1}\Psi^n}{d\,t^{n-1}}+\int m\,d\,t\,c^{-m\,t}.\frac{d^{n-1}\Psi^n}{dt^{n-1}}\right\}.$$

Now it is evident that

$$\frac{d^{n-1}\Psi^n}{d\,t^{n-1}}$$

is divisible both by t and $1-t$: it is therefore zero at both the limits of the integral; so that we have simply

$$A_{2n+1} = \frac{m}{1.2.3\ldots n}.\int_0^1 m\,d\,t\,c^{-mt}.\frac{d^{n-1}\Psi^n}{dt^{n-1}}.$$

Continuing to integrate in like manner, we shall find after n successive operations,

$$A_{2n+1} = \frac{m^n}{1.2.3\ldots n} \int_0^1 m\,d\,t\,c^{-mt}\,\Psi^n,$$

which is obviously a positive quantity.

By expanding, we get

$$\Psi^n = t^n(1-t)^n = t^n - n\,.\,t^{n+1} + n\,.\,\frac{n-1}{2}\,.\,t^{n+2} - \&c.:$$

and, by performing the differential operations,

$$\frac{1}{1.2.3\ldots n}\,.\,\frac{d^n\,\Psi^n}{d\,t^n} = 1 - n.n+1\,.\,\frac{t}{1} + \left(n\,.\,\frac{n-1}{2}\right).n+1\,.n$$

$$+ 2\,.\,\frac{t^2}{1.2} - \&c.$$

Now, because $t = \frac{x}{m}$, if we put,

$$\Psi'(x) = \frac{1}{1.2.3\ldots n}\,.\,\frac{d^n\,\Psi^n}{d\,t^n},$$

we shall have

$$\Psi'(x) = 1 - n\,.\,\frac{n+1}{m}\,.\,\frac{x}{1} + n\,.\,\frac{n-1}{2}\,.\,\frac{n+1\,.\,n+2}{m^2}\,.\,\frac{x^2}{1.2} - \&c.$$

Another form may be given to this function; for, without any variation in quantity, t and $1-t$ may be interchanged, not only in

$$\Psi^n = t^n(1-t)^n,$$

but in all its differentials, observing that the results equal in quantity will have opposite signs when the number of differentiations is odd, and the same sign when the number is even.

Now if, instead of $t = \frac{x}{m}$, we substitute $1-t = \frac{m-x}{m}$, we shall have

$$\Psi'(x) = \pm\left\{1 - n\,.\,\frac{n+1}{m}\,.\,\frac{m-x}{1}\right.$$

$$\left. + n\,.\,\frac{n-1}{2}\,.\,\frac{n+1\,.\,n+2}{m^2}\,.\,\frac{(m-x)^2}{1.2} - \&c.\right.$$

The coefficient A_{2n+1} is thus expressed in terms of x:

$$A_{2n+1} = \int_0^m dx\, c^{-x}\, \Psi'(x):$$

the indefinite integral is

$$-\, c^{-x}.\left\{\Psi'(x) + \frac{d\,.\,\Psi'(x)}{dx} + \frac{dd\,.\,\Psi'(x)}{dx^2} + \&c.\right\}.$$

This integral, taken between the limits $x = 0$ and $x = m$, is equal to A_{2n+1}: the first form of $\Psi'(x)$ will give the values of all the differentials at the limit $x = 0$; and the second form of the same function will give the like values at the other limit $x = m$: Thus we obtain,

$$A_{2n+1}\left\{1 - n\,.\,\frac{n+1}{m} + n\,.\,\frac{n-1}{2}\,.\,\frac{n+1\,.\,n+2}{m^2} - \&c.\right\}$$

$$\mp c^{-m}.\left\{1 + n\,.\,\frac{n+1}{m} + n\,.\,\frac{n-1}{2}\,.\,\frac{n+1\,.\,n+2}{m^2} + \&c.\right\},$$

the upper or lower sign taking place according as n is even or odd.

The numerical coefficients, computed by the formula, are as follows:

$$c^{-m} = c^{-10} = \cdot 0000454$$

$$A_1 = 1 - c^{-m} = 0\cdot 9999546$$

$$A_3 = \frac{4}{5} + \frac{6}{5}\,c^{-m} = 0\cdot 8000545$$

$$A_5 = \frac{13}{25} - \frac{43}{25}\,c^{-m} = 0\cdot 5199219$$

$$A_7 = \frac{7}{25} + \frac{73}{25}\,c^{-m} = 0\cdot 2801326$$

$$A_9 = \frac{16}{125} - \frac{726}{125}\,c^{-m} = 0\cdot 1277363$$

$$A_{11} = \frac{31}{625} + \frac{8359}{625}\,c^{-m}\ 0\cdot 0502072$$

$$A_{13} = 0\cdot 0172805$$

$$A_{15} = 0\cdot 0052779$$

$$A_{17} = 0\cdot 0014467$$

$$A_{19} = 0\cdot 0003593$$

$$A_{21} = 0\cdot 0000815$$

$$A_{23} = 0\cdot 0000170$$

$$A_{25} = 0\cdot 0000036.$$

The horizontal refraction answers to $\cos\theta = 0$, $e = 1$; and the part of it depending on Q_0 is found by adding all the coefficients, viz.

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} \times 2{\cdot}8024736 = 2036''{\cdot}52.$$

If we take the integral between the limits $x = 0$, $x = \infty$, the result is not sensibly different, viz.

$$\alpha(1+\alpha)\int_0^{\infty}\frac{dx\,c^{-x}}{\sqrt{2ix}} = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} = 2036''{\cdot}52.$$

Investigation of $\lambda \times Q_1$.

For this purpose we must find the value of

$$\int_0^m \frac{2\,dx\,c^{-2x}}{\Delta} = \int_0^m \frac{2\,dx\,c^{-2x}}{\sqrt{(1-e^2)^2+4e^2\cdot\frac{x}{m}}}$$

$$= \int_0^m \frac{2\,dx\,c^{-2x}}{\sqrt{(1-e^2)^2+4e^2\cdot\frac{2x}{2m}}}:$$

this integral has therefore the same form as Q_0, the quantities $2x$ and $2m$ taking the place of x and m. Wherefore, if we assume

$$e \times \int \frac{2\,dx\,c^{-2x}}{\Delta} = \mathfrak{a}_1 e + \mathfrak{a}_3 e^3 + \mathfrak{a}_5 e^5 + \&c.,$$

the value of $\mathfrak{a}_{2n+1}$ will be found merely by writing $2m$ for m in the expression of A_{2n+1}; but as $c^{-2m} = c^{-20}$ is extremely minute, the part multiplied by it may be neglected. Thus,

$$\mathfrak{a}_{2n+1} = 1 - n\cdot\frac{n+1}{2m} + n\cdot\frac{n-1}{2}\cdot\frac{n+1\cdot n+2}{(2m)^2} - \&c.$$

The numerical coefficients are as follows:

$$\begin{aligned}
\mathfrak{a}_1 &= 1\\
\mathfrak{a}_3 &= 0{\cdot}9\\
\mathfrak{a}_5 &= 0{\cdot}73\\
\mathfrak{a}_7 &= 0{\cdot}535\\
\mathfrak{a}_9 &= 0{\cdot}3555\\
\mathfrak{a}_{11} &= 0{\cdot}21505\\
\mathfrak{a}_{13} &= 0{\cdot}118945\\
\mathfrak{a}_{15} &= 0{\cdot}0604215\\
\mathfrak{a}_{17} &= 0{\cdot}0283127\\
\mathfrak{a}_{19} &= 0{\cdot}0122898\\
\mathfrak{a}_{21} &= 0{\cdot}0049621\\
\mathfrak{a}_{23} &= 0{\cdot}0018695\\
\mathfrak{a}_{25} &= 0{\cdot}0006623.
\end{aligned}$$

These values being found, if we assume

$$\lambda \times Q_1 = B_3 e^3 + B_5 e^5 + B_7 e^7 + \&c.$$

the term $\lambda \times c^{-m} \times e$, which is insensible, being omitted, we shall huve

$$\begin{aligned}
B_3 &= \lambda (a_3 - A_3) = 0{\cdot}021866 \\
B_5 &= \lambda (a_5 - A_5) = 0{\cdot}045961 \\
B_7 &= \lambda (a_7 - A_7) = 0{\cdot}055760 \\
B_9 &= \lambda (a_9 - A_9) = 0{\cdot}049829 \\
B_{11} &= \lambda (a_{11} - A_{11}) = 0{\cdot}036064 \\
B_{13} &= \lambda (a_{13} - A_{13}) = 0{\cdot}022242 \\
B_{15} &= \lambda (a_{15} - A_{15}) = 0{\cdot}012064 \\
B_{17} &= \lambda (a_{17} - A_{17}) = 0{\cdot}005878 \\
B_{19} &= \lambda (a_{19} - A_{19}) = 0{\cdot}002610 \\
B_{21} &= \lambda (a_{21} - A_{21}) = 0{\cdot}001067 \\
B_{23} &= \lambda (a_{23} - A_{23}) = 0{\cdot}000405 \\
B_{25} &= \lambda (a_{25} - A_{25}) = 0{\cdot}000144
\end{aligned}$$

By making $\cos \theta = 0$, $e = 1$, we shall have, for the approximate value of the part of the horizontal refraction depending on λQ_1,

$$\frac{\alpha (1 + \alpha)}{\sqrt{5 i}} \times 0{\cdot}253891 = 184''{\cdot}50.$$

If the integrals be taken from $x = 0$ to $x = \infty$, the same quantity will be

$$\lambda \times \alpha (1 + \alpha) \int \frac{d x \, (2 c^{-2x} - c^{-x})}{\sqrt{2 i x}} = \frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2 i}} \cdot \lambda (\sqrt{2} - 1)$$

$$= 184''{\cdot}56.$$

Between the two limits, the exact quantity obtained by integrating from $x = 0$ to $x = m = 10$, must lie; so that the error of the series is of no account.

It may be proper to make an observation here, which applies generally to the kind of integrals peculiar to this investigation. The first term of λQ_1, viz. $B_1 e = \lambda c^{-m} . e$, which is rejected, varies with the height of the atmosphere. If a small number be taken for m, that is, in low atmospheres, the refractions will vary with the height, and will not agree with the observed quantities; if a considerable number be taken, as eight or ten, or any greater number, that is, if the atmosphere extend forty or fifty miles or more above the earth's surface, the refractions will not be sensibly different from what they would be in an atmosphere of unlimited height. The invariability of the refractions concurs with other phænomena to prove that the air reaches an elevation of fifty miles, more or less.

Investigation of $f \times Q_2$.

We have

$$\frac{Q_2}{e} = \int_0^m \frac{dx}{\Delta}(4c^{-2x} - 3c^{-x} + xc^{-x}).$$

Now the following formula is easily proved by differentiating,

$$\int \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int \frac{dx\, c^{-x}}{\Delta} - \frac{m}{4} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int \frac{dx\, c^{-x}}{\Delta}$$
$$+ \frac{m}{4} \cdot \frac{1-e^2-c^{-x}\Delta}{e^2},$$

all the integrals vanishing when $x = 0$. By extending the integrals to $x = m = 10$, in which case $\Delta = 1 + e^2$, the result will be

$$\int_0^m \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int_0^m \frac{dx\, c^{-x}}{\Delta} - \frac{5}{2} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int_0^m \frac{dx\, c^{-x}}{\Delta}$$
$$+ \frac{5}{2} \cdot \frac{1-e^2+c^{-m}(1+e^2)}{e^2}:$$

and, by substituting this value, we shall have

$$Q_2 = 2e\int_0^m \frac{2dx c^{-2x}}{\Delta} - \frac{5}{2}\int_0^m \frac{e\,dx c^{-x}}{\Delta} - \frac{5}{2} \cdot \frac{(1-e^2)^2}{e^2}$$
$$\cdot \int_0^m \frac{e\,dx\,c^{-x}}{\Delta} + \frac{5}{2} \cdot \frac{1-c^{-m}}{e} - \frac{5}{2}(1+c^{-m})\,e.$$

The value of Q_2 will now be obtained in a series of the powers of e by putting for the integrals the equivalent series that have already been investigated. When this is done, the three first terms will be as follows:

$$\frac{5}{2}\left(1-c^{-m} - A_1\right) \cdot \frac{1}{e}$$
$$+\left(2a_1 - \frac{5}{2}A_1 + 5A_1 - \frac{5}{2}A_3 - \frac{5}{2}(1+c^{-m})\right) \cdot e$$
$$+\left(2a_3 - \frac{5}{2}A_3 - \frac{5}{2}A_1 + 5A_3 - \frac{5}{2}A_5\right) \cdot e^3.$$

Upon substituting the exact values of A_1, A_3, &c., the first of these terms is zero: the other two are as follows:

$$-8c^{-m} \times e$$
$$+\frac{49}{5}c^{-m} \times e^3;$$

the amount of which is very small even at the horizon; and, when multiplied by $f = \frac{2}{9}$, it becomes insensible. These terms being neglected, we may assume

$$Q_2 = C_5 e^5 + C_7 e^7 + C_9 e^9 + \&c.;$$

and we shall find

$$C_5 = 2\,a_5 - \frac{5}{2}A_5 - \frac{5}{2}\Delta^2 A_3$$

$$C_7 = 2\,a_7 - \frac{5}{2}A_7 - \frac{5}{2}\Delta^2 A_5$$

$$\vdots$$

$$C_{2n+1} = 2\,a_{2n+1} - \frac{5}{2}A_{2n+1} - \frac{5}{2}\Delta^2 A_{2n-1}.$$

The numerical coefficients will now be obtained:

$\Delta^2 A_1$	$= -\cdot 0802325$			
$\Delta^2 A_3$	$= +\cdot 0403433$	......	C_5	$= \cdot 059337$
$\Delta^2 A_5$	$= \cdot 0873930$	......	C_7	$= \cdot 151186$
$\Delta^2 A_7$	$= \cdot 0748672$	......	C_9	$= \cdot 204491$
$\Delta^2 A_9$	$= \cdot 0446024$	......	C_{11}	$= \cdot 193076$
$\Delta^2 A_{11}$	$= \cdot 0209241$	......	C_{13}	$= \cdot 142381$
$\Delta^2 A_{13}$	$= \cdot 0081714$	......	C_{15}	$= \cdot 087220$
$\Delta^2 A_{15}$	$= \cdot 0027438$	......	C_{17}	$= \cdot 046149$
$\Delta^2 A_{17}$	$= \cdot 0008096$	......	C_{19}	$= \cdot 021658$
$\Delta^2 A_{19}$	$= \cdot 0002133$	......	C_{21}	$= \cdot 009187$
$\Delta^2 A_{21}$	$= \cdot 0000511$	......	C_{23}	$= \cdot 003569$
$\Delta^2 A_{23}$	$= \cdot 0000105$	......	C_{25}	$= \cdot 001290.$

As the value of f is not fixed with the same certainty as that of λ, the coefficients of Q_2 have not been multiplied by f: the intention of which is to make it more easy to determine a variation of the refraction, viz. $\delta f \times Q_2$, answering to δf any variation of f that good observations may require.

The part of the horizontal refraction depending on Q_2 is

$$\frac{2}{9} \times \frac{\alpha(1+\alpha)}{\sqrt{5\,i}} \times 0\cdot 919534 = 148''\cdot 51.$$

If we integrate the original expression of Q_2 from $x = 0$ to $x = \infty$, c being 1, we shall have

$$\frac{2}{9} \times \alpha\,(1+\alpha) \times \int \frac{d\,x\,(4\,c^{-2x} - 3\,c^{-x} + x\,c^{-x})}{\sqrt{2\,i\,x}}$$

$$= \frac{2}{9} \cdot \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2\,i}} \cdot \left(2\sqrt{2} - \frac{5}{2}\right) = 148''\cdot 63.$$

It thus appears that the error is less than $0''\cdot 12$; for the exact integral from $x = 0$ to $x = m = 10$ is less than the second number, and greater than the first on account of the terms of the series left out.

The next point that should engage attention is to find the value of $f' \times Q_3$. In the present state of our knowledge

of the phænomena of the atmosphere, it seems impossible to determine f' by experiments. The probability is, that it is much less than f or $\frac{2}{9}$; and as the integral Q_3 is inconsiderable except within a degree or two above the horizon, and even at such low altitudes is not great; it follows that the part of the refraction depending on $f' Q_3$ will only be sensible, if at all, when a star is distant 88° or more from the zenith. At present the probability is, that there is no other way of ascertaining the value of f' but by good observed refractions at great distances from the zenith; which observations are neither numerous nor easily collected. From the uncertainty of the term $f' \times Q_3$, it cannot be estimated in constructing a table of mean refractions, which must therefore be deduced entirely from the other three terms, as in the paper of 1823. In this manner has the table in this paper been computed, by means of the formulas now to be explained. But the term $f' Q_3$ will afterwards be discussed, and its value investigated, in order that it may be taken into account, if this should be found necessary, in the progressive improvement of the theory.

When the term $f' Q_3$ is left out, the expression of the refraction will be

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot (Q_0 + \lambda Q_1 - f Q_2):$$

and if the equivalent series be substituted for the first two terms, and the series for Q_2 be multiplied by $f = \frac{2}{9}$, the result will be

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \left\{ \begin{array}{l} e \\ + 0.821921 \,.\, e^3 \\ + 0.552697 \,.\, e^5 \\ + 0.302296 \,.\, e^7 \\ + 0.132123 \,.\, e^9 \\ + 0.043365 \,.\, e^{11} \\ + 0.007883 \,.\, e^{13} \\ - 0.002040 \,.\, e^{15} \\ - 0.002930 \,.\, e^{17} \\ - 0.001842 \,.\, e^{19} \\ - 0.000893 \,.\, e^{21} \\ - 0.000371 \,.\, e^{23} \\ - 0.000139 \,.\, e^{25} \end{array} \right.$$

To bring this formula to a form more convenient for calculation, all the coefficients must be reduced to seconds. The negative terms are all very small, never amounting to so much as 6″, and of no account whatever, except the apparent altitude be equal to 2° or less; it will therefore be proper to

separate these terms from the rest, representing their sum by the symbol V (θ). These things being attended to, we have, in the first place, this formula for computing e, viz.

$$\log . \tan \phi = \log . \sec . \theta + 19{\cdot}2067840 - 20 : e = \tan \frac{\phi}{2}.$$

Next, reducing the arcs to seconds,

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} = 726''{\cdot}687:$$

$$\begin{array}{llll} & & & \text{log.} \\ \delta\theta = \sin\theta \times \Big\{ & e \times & 726''{\cdot}687, & 2{\cdot}8613472 \\ & + e^3 \times & 597{\cdot}280, & 2{\cdot}7761772 \\ & + e^5 \times & 401{\cdot}638, & 2{\cdot}6038343 \\ & + e^7 \times & 219{\cdot}674, & 2{\cdot}3417796 \\ & + e^9 \times & 96{\cdot}012, & 1{\cdot}9823255 \\ & + e^{11} \times & 31{\cdot}513, & 1{\cdot}4984866 \\ & + e^{13} \times & 5{\cdot}728, & 0{\cdot}7580287 \\ & - \mathrm{V}(\theta). & & \end{array}$$

$$\begin{array}{llll} & & & \text{log.} \\ \mathrm{V}(\theta) = \sin\theta \times \Big\{ & e^{15} \times & 1''{\cdot}483, & 0{\cdot}1710 \\ & + e^{17} \times & 2{\cdot}129, & 0{\cdot}3282 \\ & + e^{19} \times & 1{\cdot}337, & 0{\cdot}1266 \\ & + e^{21} \times & 0{\cdot}649, & -1{\cdot}8122 \\ & + e^{23} & 0{\cdot}270, & -1{\cdot}4307 \\ & + e^{25} & 0{\cdot}102, & -1{\cdot}0072 \end{array}$$

When $\theta = 87°$, V (θ) is zero; and if this function be computed for every succeeding half-degree, the quantity answering to any intermediate value of θ will be found by an easy interpolation. Such is the intention of the following table; by the help of which any refraction from the zenith to the horizon may be computed by a series of the simplest form, and consisting of no more than seven terms.

θ.	V ($\delta\,\theta$.)
87½	0''·06
88	0·14
88½	0·38
89	0·86
89½	2·30
90	5·97

If $e = 1$, the result will be the horizontal refraction, viz.

$$2078''{\cdot}53 - 5''{\cdot}97 = 2072''{\cdot}56,$$

which is almost exactly the same with 2072''·46, the quantity before computed in § 10 by a very different method.

[To be continued.]

LXXVI. *Meteorological Observations during a Residence in Colombia between the Years* 1820 *and* 1830. *By* Colonel Richard Wright, *Governor of the Province of Loxa, Confidential Agent of the Republic of the Equator, &c. &c.*

[Continued from p. 380, and concluded.]

Sanmillan; suburbs of Quito, on the plain of Anaquito, about half a mile north of the City. Situation open.

Date.	Thermometer.	Time.	Hygrometer.	Time.	Remarks.
1827.	°		°		
May 26.	”	”	5·5	8 a.m.	Rain.
27.	”	”	55·0	12	Cloudy.
28.	45·0	7 a.m.			Fair.
”	61·0	2 p.m.			
29.	43·0	6 a.m.			
”	63·0	12	27·5	12	Showery.
30.	56·0	8 a.m.			
”	64·0	2 p.m.	22·0	2 p.m.	Fair.
31.	52·0	6 a.m.			
”	66·0	3 p.m.	27·5	3 p.m.	Id.

4 days. 62°·5 max. 46°·66 min. } 54°·58 med. Hygrom. 27°·5 Var. 49°·5.

			Hygrometer.		
June 1.	53·0	7 a.m.	58·0		Cloudy; rain at night.
2.	63·0	2 p.m.	...	22·0	
3.	52·0	7 a.m.	57·5		
”	63·0	2 p.m.	...	27·5	Cloudy.
4.	48·0	6 a.m.	56·5		
”	65·5	2 p.m.	...	38·5	Fair.
5.	48·0	6 a.m.	57·0		
”	66·0	2 p.m.	...	38·5	Showery.
6.	52·0	6 a.m.	58·5		
7.	65·0	2 p.m.	...	38·5	Id.
8.	63·0	2 p.m.	...	16·5	Id.
9.	64·0	2 p.m.	...	55·0	Fair.
10.	53·0	7 a.m.	59·0		
”	65·0	2 p.m.	...	55·0	Id.
11.	43·0	6 a.m.	...		
”	54·0	8	56·25		
”	65·5	2 p.m.	...	55·0	Id.
12.	52·0	7 a.m.	58·5		
”	65·0	2 p.m.	...	49·5	Id.
13.	67·0	2 p.m.	...	55·0	Cloudy.
14.	51·0	7 a.m.	57·5		
”	76·0	sun			
”	64·0	2 p.m.	...	44·0	Cloudy; drops of rain.
15.	52·0	6½ a.m.			
”	57·0	7	58·5		Fair.
”	59·5	8½			
”	60·0	9½	...		

Sanmillan.—TABLE *continued.*

Date.	Thermometer.	Time.	Hygrometer.		Remarks.
1827.			°		
June 15.	64·0	2½ p.m.	...	66·0	
16.	55·0	6½ a.m.			
"	58·0	7½			
"	62·0	9½	52·2		
"	66·0	2 p.m.			
"	80·0	sun.			
18.	48·0	6½ a.m.			
"	61·0	sun.	54·83		Windy and light breeze.
"	53·0	8			
"	63·5	3 p.m.	...	66·0	
19.	49·0	7 a.m.			
"	55·5	9	57·16		Fair.
"	75·0	sun.			
"	67·0	2 p.m.			
20.	48·0	6½ a.m.			
"	61·0	sun.	56·66		
"	58·0	9			
"	74·0	sun.			Id.
"	64·0	2 p.m.	...	60·5	
"	82·0	sun.			
21.	45·0	6½ a.m.			
"	55·0	9	54·66		Id.
"	74·0	sun.			
"	64·0	3½ p.m.	...	44·0	
22.	44·0	6 a.m.			
"	52·0	7	55·25		Id.
"	59·0	10			
"	66·0	2 p.m.	...	38·5	During a hail storm.
"	82·0	sun.			
23.	54·0	8 a.m.	59·0		
"	71·0	sun.			Cloudy and windy.
"	64·0	3 p.m.	...	49·5	
"	80·0	sun.			
24.	53·0	7 a.m.	58·5		Slight showers.
"	64·0	2 p.m.	...	44·0	
25.	49·0	6 a.m.			Fair.
"	56·0	9	52·5		
"	75·0	sun.			
26.	51·5	6½ a.m.	58·75		
"	66·0	3 p.m.	...	55·0	Id.
27.	53·0	7 a.m.	60·0		Clear and strong winds from N.E.
"	67·0	3 p.m.	...	66·0	
28.	55·0	7 a.m.	59·5		
"	64·0	3 p.m.	...	55·0	Cloudy and showery.
29.	50·0	7 a.m.			
"	60·0	10			
"	64·0	12	59·0		
"	62·0	2 p.m.	...	44·0	Id.

Sanmillan.—TABLE *continued.*

Date.	Thermometer.	Time.	Hygrometer.		Remarks.
1827. June 30.	° 48·0	7 a.m.	55·87	22·0	Cloudy and damp.
„	54·5	9			
„	56·0	9½			
„	74·0	sun.			
„	65·0	3 p.m.	...	49·5	Fair.
„	81·0	sun.			
July 1.	49·0	7 a.m.			
„	59·0	10	...	36·5	Fair.
„	63·0	3 p.m.	55·25	49·5	Id.
„	60·0	10			
2.	53·0	7 a.m.			
„	65·0	2 p.m.	59·33	55·0	Cloudy.
„	60·0	8			
3.	54·0	7 a.m.			
„	64·0	sun.			
„	62·0	9	...	66·0	Clear.
„	74·0	sun.	60·33		
„	66·0	2½ p.m.	...	77·0	Id. and wind.
„	84·0	sun.			
„	53·0	10			
4.	53·0	7 a.m.			
„	65·0	3½ p.m.	57·0	66·0	Clear.
„	53·0	7			

34 days.—Thermomer mean 57°·25.—Hygrometer 47°·75. Var. 60°·5.

August 31 days.	67°·37 max. 52 ·5 min.	59°·93 med. Hygr. 61°·88. Var. 49°·5. Weather generally fair and windy.
Septemb. 30 days.	67°·0 max. 54 ·54 min.	60°·77 med. Hygr. 59°·43. Var. 72°·5. Fair at the beginning; afterwards cloudy with some showers.
October 12 days.	61°·7 max. 53 ·0 min.	57°·35 med. Hygr. 28°·15. Var. 27°·5.

Dec. 1.	51·0	6 a.m.	61·75		Mornings clear: evenings cloudy.
„	72·5	2 p.m.			
2.	51·0	6 a.m.			
„	74·0	2 p.m.	62·0		
„	61·0	5			
7.	51·0	6 a.m.			
„	71·0	2½ p.m.	62·0		Cloudy; rain at night.
„	64·0	5½			

Sanmillan.—TABLE *continued.*

Date.	Thermometer.	Time.	Hygrometer.		Remarks.
1827.					
Dec. 8.	72°·0	2 p.m.			Showery.
9, 10.	70·0	2 p.m.			Fair.
11,12,13.	72·0	2 p.m.			Id.
14.	74·0	2 p.m.			
„	127·0	reflected	heat.		
15.	52·0	6 a.m.			
„	73·0	2 p.m.	60·25		
„	60·0	4			
„	56·0	5			Storm.
16.	69·0	3 p.m.			Id.
17,18,19.	68·0	3 p.m.			Fair.
20.	51·0	6 a.m.			
„	55·0	8	58·66		Id.
„	70·0	2 p.m.			9 p.m. shock of an earthquake and rain.
21.	72·0	2 p.m.			
22, 23.	72·0	2 p.m.			Fair.
24, 25.	74·0	2 p.m.			
26.	71·0	2 p.m.			Id.
27.	72·0	2 p.m.			Id.
28.	69·0	2 p.m.			Id.
29.	51·0	6 a.m.			
„	71·0	2 p.m.	60·66		Rain.
30.	60·0	5 p.m.			

25 days. 60°·88 mean. Var. 22°. Maximum of heat 74°·0: extraordinary in Quito. Vide Humboldt, *De Geog. Plant.*, p. 103.

Average temperature of the neighbourhood of Quito from May to December 58°·7. Hygrometer of Leslie 42°·03.

Comparative Temperature of different Parts of the Table-land of the Equator.

	Quito.		Ambato.	
1825.				
Aug. 12.	58·0	6 a.m.	58·0	
„	62·5	2 p.m.	66·0	
13.	57·5	6 a.m.	58·0	
„	61·5	2 p.m.	65·0	
14.	56·0	6 a.m.	58·0	
„	67·0	2 p.m.	64·0	

$\left.\begin{matrix}63°·66\\57·16\end{matrix}\right\}$ 60°·41 $\left.\begin{matrix}65°·08\\58·00\end{matrix}\right\}$ 61°·5. Difference 1·09.

Comp. Temp.—Table *continued.*

	Quito.		Riobamba.	
1825. Aug. 17.	56·5	6 a.m.	57·0	
"	65·0	2 p.m.	65·0	
18.	56·5	6 a.m.	53·0	
"	63 0	2 p.m.	60·0	
19.	54.0	6 a.m.	53·0	
"	64·0	2 p.m.	60 0	
20.	55·0	6 a.m.	53·0	
"	65·0	2 p.m.	60·0	
21.	55·0	6 a.m.	53·0	
"	65·0	2 p.m.	65·0	
	64°·4 55 ·5	} 59°·95	62°·0 53 ·8	} 57°·9 : difference 2°·05.
	Quito.		Guaranda.	
24.	57·0	6 a.m.	56·0	
"	67·0	2 p.m.	63·0	
25.	57·5	6 a.m.	55·0	
"	67·0	2 p.m.	63·0	
26.	58·0	6 a.m.	56·0	
"	66·5	2 p.m.	65·0	
27.	57·0	6 a.m.	56·0	
"	67·0	2 p.m.	65·0	
28.	67·5	2 p.m.	66·0	
31.	58·0	6 a.m.	56·0	
"	67·0	2 p.m.	68·0	
	67°· 57 ·5	} 62°·25	65°·33 55 ·8	} 60°·56 : difference 1°·69.

Village of Cayambe. *Lat.* 1′ 35″ *N.* *Elevation* 9724 *feet.*
B. W. 195°$\frac{9}{10}$.

Date.	Thermometer.	Time.	Hygrometer.	Remarks.
1827. July 8.	52°0	7 a.m.		
"	64·0	3 p.m.	66°·0	Fair and clear.
9.	52·0	7 a.m.		Id.
10.	57·0	7 a.m.		
"	59·0	10	47·5	
"	60·0	4 p.m.		Cloudy.
11.	64·0	2 p.m.	75·0	Fair and windy.
12.	56·5 }	8 a.m.		
"	71·0 }	sun.		
"	64·0	3 p.m.	75·0	Id.

TABLE *continued.*

Date.	Thermometer.	Time.	Hygrometer.	Remarks.
1827.	°		°	
July 16.	65·0	12	71·0	Fair and windy.
17.	64·0 }	1 p.m.	82·5	Id.
,,	84·0 }	sun.		
20.	67·0	2 p.m.	77·0	Cloudy.
21.	71·5	1 p.m.	97·1	Clear and windy.
,,	78·0	sun.		
9 days	64°·81 max. 53 ·66 min.		59°·23	73°·58. Var. 50°.

Mountain of Cayambe, under the Equator.

24.	37·0	6 a.m.	16·5	Elevation 12,705 feet. B. W. 191°. Fair.
,,	43·0	$1\frac{1}{2}$ p.m.	13·9	Elevation 14,217 feet. B. W. $188\frac{1}{2}$ foot of the Nevado. Weather fair.

Farm of Antisana.

1829.				
July 1.	33·0	6 a.m.		14,520 feet. B. W. 188°. Fair and windy.
	35·0	7		
	38·0	$8\frac{1}{2}$		
	45·0 }	$2\frac{1}{2}$ p.m.	30·3	Bright.
	67·0 }	reflected	heat.	
	43·0	4		Drifts of mists or *Parametos.*
	37·5	$5\frac{1}{2}$		
Med. 38°·58.			30°·3 Hygrometer.	

Descent from the Table-land of Quito to Esmeraldas, and Coast of the Pacific.

Date.	Thermometer.	Time.	Hygrom.	Elevation.	Remarks.
1828.			°		
May 14.	53·0 }	12		12,986 ft. B.W. 190°$\frac{1}{4}$.	Height of Pichan.
,,	83·0 }	sun.			
15.	64·0	3 p.m.	30·3	8772 feet. B.W. $197\frac{1}{2}$°.	Descent of Pugsi.
17.	65·5	mean of 10 obs.		3932 B.W. $205\frac{1}{2}$°.	Mindo.

Descent from Table-land.

Table *continued.*

Date.	Thermometer.	Time.	Hygrom.	Elevation.	Remarks.
1828.	°		°		
May 26.	...	...	...	3025 B.W. 207.	Bola nignas.
27.	70·0	8 a.m.		2728 B.W. 207½°.	Casha ponga.
29.	72·0	6 a.m.		1210 B. W. 210.	Palo grande.
„	78·0	3 p.m.			
30.	72·0	6 a.m.	fair.		
„	74·0	2 p.m.			
31.	74·0	2 p.m.	rain.		
June 19.	73·0	8 a.m. }	fair.	605 feet, B.W. 211°.	Canigue, about 12 leagues from the coast, surrounded by deep forests.
„	73¾	river. }			
„	75·0	9			
„	77·0	10½			
„	79·0	1 p.m. }			
„	76·0	river. }	22·0		
„	80·0	2			
„	82·0	3	bright.		
„	76·0	8	33·0		
20.	73·0	6 a.m. }	11·0		
„	73·0	river. }	cloudy.		
„	72·5	8½			
„	74·0	9½ }	misty &		
„	74¾	river. }	wind.		
„	76·0	11			
„	78·0	12	showery.		
„	82·0	2½ p.m. }	fair.		
„	77·0	river. }			
„	83·0	4½			
„	80·0	5½			
„	76·0	7			
21.	72·0	6½	rain.		
„	77·0	10½	cloudy		
„	77½	12	and		
„	79·0	1 p.m.	wind.		
„	78·5	3½	22·0		
„	73·0	7 p.m.			
22.	70·0	6 a.m.			
„	74·0	9			
„	78·0	12 }	cloudy.		
„	76·5	river. }	19·2		
„	79·5	4 p.m.	breeze	and mist.	
23.	73·0	7 a.m.			
„	80·5	1 p.m.	fair.		

Med. 74°·56. Water 71°·83. Hygrometer 22°·77.
Thermometrical var. 10°50·. Hygrometrical do. 22°.

Observations at Canigue, by Col. Brooke Young.

1828.	6 a.m.	Noon.	3 p.m.	6 p.m.	Medium.	Weather.
	°	°	°	°		
Jan. 19.	74·0	82·5	82·0	76·0	78½	Continual rain.
20.	73·0	81·0	79·0	75·0	77·0	Heavy showers.
21.	72·0	85·0	83·0	77·0	79¼	Light showers.
Feb. 2.	72·0	85·0	84·0	76·0	79¼	Heavy showers.
3.	71·0	86·0	90·0	81·0	82·0	Fair.
4.	75·0	78·0	78·0	76·0	79¼	Continual rain.
5.	73·0	81·0	84·0	77·0	78¼	Fair until 5 p.m.
6.	72·0	83·0	86·0	78·0	79¾	Do.
7.	73·0	79·5	80·0	77·5	77½	Heavy rain.
8.	73·5	83·0	84·0	78·5	79¾	Light showers.
9.	73·0	86·0	86·0	77·0	80½	Rain after 5 p.m.
10.	73·0	83·0	87·0	80·0	80¾	Fair.
11.	74·0	80·5	85·0	77·0	79⅛	Rain until 10 a.m.
12.	73·0	83·0	85·0	81·0	80½	Do.
13.	73·5	86·0	86·0	77·5	80¾	Showers after 3 p.m.
14.	74·0	82·0	83·0	77·0	79·0	Showery.
15.	75·0	83·0	86·5	79·0	80⅞	Rain after 6 p.m.
16.	74·0	79·0	79·0	76·0	77·0	Heavy showers.
17.	73·0	80·0	87·0	79·0	79¾	Showers after 4 p.m.
18.	75·0	84·0	89·0	79·0	80⅞	Fair.
19.	75·0	85·0	88·0	79·0	80⅞	Id.
20.	73·0	83·0	82·0	79·0	79¼	Showers after 2 p.m.
21.	73·0	84·0	89·0	81·0	81⅞	Rain after 6 p.m.
22.	72·0	80·0	85·0	81·0	79½	Rain all night.

Medium of the rainy season 79°0 nearly.
——— of the dry season 74° 56.
——— of the year...... 76 ·78.

River of Esmeraldas.

1828.	Air.	Water.	Hygrom.	Time.	Weather.
June 12.	76°0	75·5	5°5	6¾ a.m.	Misty rain.
,,	78·0	77·5	0·0	3½ p.m.	Do.
13.	76·0	75·0	0·0	6½ a.m.	Do.
,,	79·0	76·0	5·5	10½	Cloudy.
,,	79·5	76·0	8·2	3 p.m.	Do.
14.	75·0	74·5	0·0	6½ a.m.	Misty.
,,	80·5	75·0	11·0	10	Fair.
15.	80·0	76·0	13·7	12	Id.
16.	80·5	74·5	13·7	10 a.m.	Id.
18.	73·0	73·0	0·0	6½ a.m.	Misty.
Med.	77·75	75·4	5·76		

Town of Esmeraldas. *Lat.* 55′ *N.*

1828.	Therm.		Hygrom.	Time.	Weather.
June 1.	76°·0			7 a.m.	About six miles from the sea-coast. Elev. 0.
	77·0			8	Cloudy.
	80·0			9	Do.
	82·0			10	Do.
Med. 81°·5.	85·0			11	Do.
	87·0		°	1 p.m.	Do.
	84·0			2	Do.
	82·0		22·0	3	Rain.
	82·0			4	Do.
	80·0			6	Do.
2. 79°·62.	76·0			8 a.m.	Cloudy.
	78·0			10	Do.; rain.
	82·5		19·2	1 p.m.	Do.
	82·0			3	Misty.
3.	76 0			7½ a.m.	Do.
	80 5		16·7	3 p.m.	Do.
4. 78°·95.	76 0			7 a.m.	Fair.
	80·0			10	Do.
	85·0			1 p.m.	Do.
	78 0			9	Do.
5. 79°·75.	84·0		27·5	2 p.m.	Do.
	76 0			6½	Do.
8. 80°·0.	75·0			6½ a.m.	Misty.
	78·0			9½	Fair.
	83·0		27·5	11½	Do.
	84·0			1½ p.m.	Do.
	82·0			3½	Do.
	76·5			9	Do.
9. 78·35.	75·0	Sun 84°.		7 a.m.	Do.
	76 0			8	Do.
	78·0			9	Do.
	81·0		22·0	10	Do.
	83 0			11	Do.
78°·75.	82·0			12	Cloudy.
	80·3			1 p.m.	Rain.
	78·0			3	Heavy rain.
	78·0			4	Do.
	78·0			5	Do.
10.	77·0			8	Fair.
	76·0			7 a.m.	Do.
	80·5			10	Do.
	83·0			12	Cloudy.
	83·0		27 5	2 p.m.	Do.
	80·5			4	Fair.
	Med. 79·65.	21°·78.		Thermometrical Var. 10°. Hygrometrical do. 15·6.	

Sea-coast South of the River of Esmeraldas.

1827.	Therm.		Hygrom.	Time.	Weather.
May 20.	75°0		B.W. 212°.	7 a.m.	Atacames. Lat. 52′ N. Cloudy.
	78·0			4 p.m.	Cloudy.
21.	75·0			6 a.m.	
	77·5			12	
Dec. 15.	76·4		22°0	8 p.m.	Cloudy.
16.	72·6		6·5	6 a.m.	Rain.
	81·5		19·0	2 p.m.	Cloudy.
18.	79·0		20·0	3 p.m.	*Mompiche.*
19.	75·5		5·5	6 a.m.	Cloudy; rain at night.
„	80·0		30·0	2 p.m.	Clear.
1830.	72·0			6 a.m.	*River Muisne.*
Nov. 7.	76·0			3 p.m.	
8	72·0			6 a.m.	
„	79·0			3 p.m.	
9, 10.	72·0			6 a.m.	Wind, cloudy and mists.
11.	76·0			3 p.m.	
12.	72·0		22·0	6 a.m.	
„	77·0			3 p.m.	
13.	71·0			6 a.m.	Rain.
	75·0			3 p.m.	
	76°·28				

LXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Address of the Marquis of Northampton, *President, read at the Anniversary Meeting, November* 30, 1839.

Gentlemen,

A YEAR having now elapsed since you conferred upon me the highly honourable office of your President, it becomes my duty, in accordance with the example of my predecessors, to address you. The first and most agreeable part of my task is to express my feelings of gratitude to those Gentlemen whom you were pleased to select as my Council. I am most highly indebted to them for the zealous co-operation and hearty assistance which I have ever received at their hands. It is to them that I have looked to aid my inexperience, and to supply my manifold deficiencies, and I have not been disappointed. To those who are the more permanent officers of the Society, the Treasurer and the Secretaries, my obligations are particularly great, and I will venture to add, that to them, as well as to the other members of the Council, your thanks are due as well as mine.

The past year has indeed been to that portion of the Royal So-

ciety which takes an active part in its affairs, one of more than usual labour and exertion,—of labour and exertion, destined, as I hope, to produce rich and ample fruit. The great and marking peculiarity which has attended it, has been the sailing of the Antarctic Expedition. The importance of following up in the southern regions of the globe the magnetic inquiries so interesting to men of science in Europe, was strongly felt by one of our distinguished Fellows, Major Sabine, and by him brought before the notice of the British Association at their meeting at Newcastle, as he had also previously done at Dublin. That great assemblage of men of science, concurring in the views of Major Sabine, resolved to suggest to Her Majesty's Government the propriety of sending out a scientific expedition; and the Royal Society lost no time in warmly and zealously seconding the recommendation. It would, Gentlemen, be an idle inquiry to ask whether the success of the application be owing to the British Association or to the Royal Society. It would seem, indeed, probable, that, considering the financial difficulties of the time, the Government might have hardly considered itself justified in yielding to the prayers of either body separately on this occasion; and if to the British Association be the glory of the first proposal of this Expedition, to the Royal Society belongs the praise of perseverance in seconding the recommendation, and of laborious and earnest endeavours to aid in rendering it in every respect as efficient as possible. It is my duty as your President to return my thanks and yours to Lord Melbourne, Lord Minto, Lord Monteagle, Sir Hussey Vivian, and Sir Richard Jenkins, the Chairman of the Honourable Court of Directors of the East India Company, for the urbanity and kindness with which they have received and acted on the suggestion of your Council, and for the confidence which the Government reposed in us, when they asked for our assistance in instructing the officers to whom the Expedition has been intrusted.

In compliance with the request conveyed to us by the First Lord of the Admiralty, the Council transmitted to the Government a body of hints and instructions in different branches of science, which I trust are likely to be of material use both to the principal and to the subsidiary objects of the Antarctic Expedition*.

These hints and instructions would have been far less extensive and efficient if the Council had not been able to have recourse to the several Scientific Committees, of whose formation the Society is already aware. The Expedition has now sailed, amply provided with the best scientific instruments and furnished with ample scientific instructions: it is commanded by one well acquainted both with magnetic inquiry and nautical research. We may therefore hope that, with the blessing of Providence, it will return with a store of knowledge valuable to the geographer, to the geologist, to the meteorologist, and to him also who studies the marvels of vegetable and animal life. In addition to all this we may hope, that the main ob-

[* Part of these instructions will be found in the present volume, p. 177; the remainder will appear in the next volume of the Annals of Natural History.—Edit.]

ject of the Expedition will be accomplished by additional light thrown on the obscure problems which still attend the magnetism of the earth, and that by such discoveries Captain James Clark Ross may not only add to his own reputation and his country's glory, but also give to the adventurous mariner increased facility and security in traversing the pathways of the ocean.

The Antarctic Expedition was not the only measure recommended by the Royal Society and the British Association to Her Majesty's Government. Another important recommendation, which had previously been brought forward by Baron Humboldt, was the establishment of fixed magnetic observatories for the purpose of making simultaneous observations in different parts of our colonial possessions. These recommendations have been readily acceded to, both by the Government and by the Court of Directors of the East India Company, and probably, ere many months shall have elapsed, the observatories will be in full activity. This ready acquiescence in the wishes of men of science appears to me highly creditable to our statesmen; and I feel confident, that while science belongs to no party, on the other hand, every party in this country is fully aware of the importance of science, and of the numerous benefits conferred by it on the human race*.

I have stated, Gentlemen, that your Council had recourse to the Scientific Committees for assistance in drawing up instructions for the Expedition in different branches of knowledge; those committees, who were named only two years ago, were at first apparently more a matter of form than substance; they have now been found capable of doing excellent service. Not only has your Council consulted them on the questions already alluded to, but also, observing that the several Committees are composed of the most competent judges of the merits of the memoirs in the respective departments of science communicated to the Society, they have, in general, referred the papers to them to report upon, previously to coming to a decision regarding their publication.

The Royal Society, from its character of pursuing every branch of physical science, is evidently in a different position from other societies professing some one science alone. It may be reasonably expected, that in the Botanical or Geological Society, for instance, the whole Council should possess a certain degree of botanical or geological knowledge. This, however, cannot be the case with us. Our Council will comprise a few astronomers, a few zoologists, a few botanists, and a few persons well acquainted with geology and medicine; but no single science can monopolize a large number of its members. In difficult questions we have therefore felt that it is more satisfactory to ourselves, and we think probably more so to the general body of the society, and to those who have favoured us with papers, that we should ask the opinion of a larger number of men conversant with the immediate sciences in question. At the same time, the Council retains its responsibility for its acts, and the

[* See L. & E. Phil. Mag. vol. ix. p. 42. The instructions will be found at p. 224. of the present volume.—Edit.]

chief officers of the society are officially members of each of the scientific committees.

The Council have derived a further assistance from these Committees in the adjudication of our medals. In naming these Committees, the Council has had both a difficult and a delicate task. Convinced that bodies, when too numerous, are little adapted for business, they have also felt that the power of giving their attendance might be more important than absolute superiority of scientific attainments. Some members have, however, been selected, though really non-resident, because it was believed that their colleagues might wish to consult them by letter. With these objects and views, the Council have done their best; but they have little doubt that some gentlemen have been overlooked and omitted, whose presence in the Committees might have been very desirable. The Society must consider this as in some degree a new system, to be perfected and improved by experience alone.

Another question has occupied a share of the time of the Council during the last year. We have felt that the testimonial of recommendation for new Fellows has scarcely been sufficiently definite and precise in stating the grounds on which the candidate was recommended to the body of the Society. We have therefore thought it desirable to draw up forms of testimonial, some one of which may be adopted as most fit for each individual so recommended. We have thought this more fair, at the same time, to the meritorious candidate and to those electors who are otherwise left in the dark with respect to his claims for their suffrages. We hope and trust that this new regulation will not stand in the way of any candidate who would be a desirable addition to our number.

The labours of our tried and valuable officer, Mr. Roberton, having materially increased, partly in consequence of the establishment of Scientific Committees, and partly from other causes, and those labours having also become more valuable from the lengthened experience of many years spent in our service, it has appeared to us an act of mere justice to augment his salary from £160 to £200 per annum.

The Society are doubtless aware, that, at the time of the last Anniversary, no final settlement had taken place of the pecuniary claim of Mr. Panizzi, who had commenced the Catalogue of our Library, though that gentleman had received a considerable sum on account. Feeling that it was very desirable to bring this question to a termination, we agreed to a reference; and Mr. Drinkwater Bethune having been proposed by Mr. Panizzi, and agreed to by us, that gentleman has decided that a balance of £328 is still due by us.

The vacancies in the list of our Foreign Members have been supplied by the election of M. Savart of Paris, Signor Melloni of Parma, M. Quetelet of Brussels, M. Hansteen of Christiana, Prof. Agassiz of Neufchatel, and M. von Martius of Münich, as those Fellows who were present at their election will remember.

I have to announce to you, Gentlemen, with great regret, the re-

tirement of Captain Smyth from the office of Foreign Secretary, in consequence of his leaving his present residence for one at an inconvenient distance from London.

I shall not detain you by any observations of the finances of the Royal Society, as you will shortly hear the report of the Treasurer on that subject.

I have the honour, Gentlemen, to inform you that the Council have, by an unanimous decision, awarded the Royal Medals to Dr. Martin Barry and Mr. Ivory, and the Copley Medal for the year to Mr. Robert Brown; and I shall now beg leave to address myself to those three Gentlemen.

Dr. Barry—it gives me sincere pleasure to bestow this medal on a gentleman who has so well deserved it, by researches in a difficult and important portion of animal physiology*. Your merits have been appreciated by men much more capable of understanding the subject than I can pretend to be—by men selected by the Council of the Royal Society for their physiological science, who have felt the great value of the discoveries you have made by accurate and diligent research, aided by the skilful use of the microscope. I trust that the award of this medal will encourage you to persevere in the same course, and that future discoveries may add to your reputation and to that of the important profession to which you belong.

Mr. Ivory—it is not the first time that you have been addressed from this chair, and it gives me great satisfaction to follow the steps

* These researches are the subject of Dr. Martin Barry's papers "On Embryology," communicated to the Royal Society in 1838 and 1839. [Abstracts of which will be found in L. & E. Phil. Mag. vol. xiii. p. 458. and vol. xiv. p. 493.]

In these memoirs the author has brought to light many new and interesting facts, and has repeated and confirmed previous observations regarding the nature, formation, and developement of the ovum in the vertebrata, and especially in the mammalia.

The importance of the subject and the difficulty of its investigation, render the establishment of facts previously known extremely acceptable to physiologists. But the novel matter contained in Dr. Barry's Memoirs forms a considerable proportion of them. Without entering into unnecessary detail, we may mention that the author has determined the order of formation of the different parts of the ovum, and the nature and mode of developement of the vesicle (ovisac), in which these processes take place. He has, in like manner, discovered the nature and traced the developement of the so-called disc of M. Baer, and has detected in it the mechanism which mainly regulates the transit of the ovum into the Fallopian tube. The second series of Dr. Barry's observations makes known the changes which the ovum undergoes in its passage through the Fallopian tube; the earliest and most interesting stages of developement being for the first time described in this memoir.

The value of his very laborious and extensive series of minute observations is greatly enhanced by the clearness and method with which the results are given, and by the comparisons, which the author's intimate acquaintance with this branch of physiological literature has enabled him to institute, between his own observations and those of his predecessors in the same branch of inquiry.

of my predecessors, Sir Joseph Banks and Sir H. Davy, by again bestowing a medal on one who is an honour to the Royal Society and pre-eminently distinguished for his mathematical attainments. The labours of your life are too well known to the scientific world to require any eulogium from me, and I consider that in this tribute to your paper on astronomical refraction, we are rather doing an honour to ourselves than to you*.

Mr. Brown—in conferring the Copley Medal on you for your valuable discoveries in vegetable impregnation†, I am quite sure that the voice of scientific Europe will respond to the decision of the Council of the Royal Society. The Académie des Sciences has already pronounced on your merits, as also on those of Mr. Ivory, by electing you as well as that gentleman to a seat among their foreign members; and the University of Oxford has also, by an honorary degree, given you a similar testimonial. That you are one of our fellows is to myself a circumstance peculiarly agreeable, as it must be to the whole body over whom I have the honour to preside. Your discoveries in the particular botanical question, for which I have to give you the Copley Medal, are so important, not only in a botanical, but also in a general scientific point of view, by showing the close analogies of animal and vegetable life, that the Committee of Zoology have felt it as much their province as that of the Committee of Botany, to recommend that the Copley Medal should be bestowed upon you; and the Council have come to an unanimous resolution to give it, though at the same time other gentlemen were recommended by other scientific committees, with whom even an unsuccessful rivalry would be no mean praise.

I hope, Mr. Brown, that you may long enjoy life and leisure to pursue researches so valuable to science and so honourable to the country of which you are a native.

In drawing up the following notice of the losses which the Royal Society has sustained during the last year, in conformity with the practice of my predecessors, I have availed myself of the assistance of one of the Fellows, whose acquaintance with the labours of men of science peculiarly qualified him for the execution of a task which I could not myself have ventured to undertake. I therefore will not longer occupy your time by any further remarks of my own, but will conclude by the expression of my present wishes for the prosperity of the Royal Society, and for its success in furthering the noble ends for which it was instituted.

The Rev. Martin Davy was originally a member of the medical

[* Mr. Ivory's paper on the astronomical refractions will be found reprinted in our last, present, and next volumes.—Edit.]

† The following are the discoveries referred to: viz., the organization of the vegetable ovule, immediately before fecundation, (published in 1826); and the direct action of the pollen, manifested by the contact established between it and that point of the ovulum where the embryo subsequently first becomes visible, and published in papers, in the years 1832 and 1833, and communicated to the Linnean Society. [See Phil. Mag. First Series, vol. lxvii. p. 352; Phil. Mag. and Annals, N. S., vol. x. p. 437; and L. & E. Phil. Mag. vol. i. p. 70.—Edit.]

profession, which he followed, during a great part of his life, with no inconsiderable reputation. He became a medical student of Caius College in 1787, and was elected to a fellowship in 1793 and to the mastership in 1803, the late illustrious Dr. Wollaston being one of his competitors. One of the first acts of his administration was to open his College to a more large and liberal competition, by the abolition of some mischievous and unstatutable restrictions, which had been sanctioned by long custom, and also by making academical merit and honours the sole avenue to college preferment: and he lived to witness the complete success of this wise and liberal measure, in the rapid increase of the number of high academical honours which were gained by members of his College, and by the subsequent advancement of many of them to the highest professional rank and eminence.

Some years after his accession to the mastership, he took holy orders and commuted the degree of Doctor in Medicine for that of Theology, and in later life he was collated to some considerable ecclesiastical preferments. Dr. Davy had no great acquaintance with the details of accurate science, but he was remarkable for the extent and variety of his attainments in classical and general literature; his conversation was eminently lively and original, and not less agreeable from its occasional tendency to somewhat paradoxical, though generally harmless speculations. He died in May last, after a long illness, deeply lamented by a large circle of friends, to whom he was endeared by his many social and other virtues.

Dr. Herbert Marsh, Bishop of Peterborough, and one of the most acute and learned theologians of his age, became a member of St. John's College in the University of Cambridge in the year 1775 and took his B.A. degree in 1779, being second in the list of Wranglers, which was headed by his friend and relation Mr. Thomas Jones, a man whose intellectual powers were of the highest order, and who for many years filled the office of tutor of Trinity College with unequalled success and reputation. Soon after his election to a fellowship, he went to Germany, where he devoted himself during many years to theological and general studies, and first became known to the public as the translator and learned commentator of Michaelis's Introduction to the New Testament. It was during his residence abroad that he published in the German language various tracts in defence of the policy of his own country in the continental wars, and more particularly a very elaborate "History of the Politics of Great Britain and France, from the time of the Conference at Pilnitz to the Declaration of War," a work which produced a marked impression on the state of public opinion in Germany, and for which he received a very considerable pension on the recommendation of Mr. Pitt. In 1807, he was elected Lady Margaret's Professor of Divinity in the University of Cambridge, an appointment of great value and importance, which he retained for the remainder of his life. On the resumption of his residence in the University, he devoted himself with great diligence to the preparation of his lectures on various important branches of Divinity, interposing a great number of occasional publications on the Catholic Question, the Bible So-

ciety, and various other subjects of political and theological controversy. In 1816 he was appointed Bishop of Llandaff; and three years afterwards he was translated to the see of Peterborough. In the course of a few years from this time, his health, which had been already undermined by his sedentary habits and severe studies, began rapidly to decline, and he was compelled to abstain from the active duties of his professorship and from the exciting labours of controversy; and though his infirmities continued to increase both in number and severity, yet his life was prolonged to a mature old age by the vigilant and anxious care and nursing of one of the most exemplary and affectionate of wives.

Dr. Marsh was a man of great learning and very uncommon vigour of mind, and as a writer, remarkable for the great precision of his language and his singular clearness in the statement of his argument. His lectures on Divinity are a most valuable contribution to the theological student, and his "Comparative View of the Churches of England and Rome" presents one of the most masterly views of the great principles which distinguish those churches, which has ever appeared from the pen of a Protestant writer. His controversial writings, though generally full of acuteness and ability, must be expected to share the fate of all productions which are not kept from perishing by the permanent existence of the interests, of whatever nature, which gave rise to them: and we may justly lament that learning and powers of reasoning of so extraordinary a character, were not more exclusively and steadily devoted to the completion of more durable and systematic theological labours.

The father of the late Professor Rigaud had the care of the King's Observatory at Kew, an appointment which probably influenced the early tastes and predilections of his son. He was admitted a member of Exeter College, Oxford, in 1791, at the early age of sixteen, and continued to reside there as fellow and tutor until 1810, when he was appointed Savilian Professor of Geometry. He afterwards succeeded to the care of the Radcliffe Observatory, and the noble suite of instruments by Bird, with which it is furnished, was augmented, on his recommendation, by a new transit and circle, so as to fit it for the most refined purposes of modern practical astronomy: and we venture to express a hope that it will shortly become equally efficient and useful with the similar establishment which exists in the sister university.

Professor Rigaud published, in 1831, the miscellaneous works and correspondence of Bradley, to which he afterwards added a very interesting supplement on the astronomical papers of Harriott. In 1838, he published some curious notices of the first publication of the Principia of Newton; and he had also projected a life of Halley, with a view of rescuing the memory of that great man from much of the obloquy to which it has been exposed; he had made extensive collections for a new edition of the mathematical collections of Pappus; and he was the author of many valuable communications to the Transactions of the Royal Astronomical Society, and to other scientific journals, on various subjects connected with physical and astronomical science. There was probably no other person of

his age who was equally learned on all subjects connected with the history and literature of astronomy*.

Professor Rigaud was a man of most amiable character, and of singularly pleasing manners and person. The warmth of his affections, his modesty, gentleness, and love of truth, as well as the great variety of his acquirements and accomplishments, had secured him the love and the respect of a large circle of friends, not merely in his own university, but amongst men of science generally. He died in London in March last, after a short but painful illness, which he bore with a fortitude and resignation which might have been expected from his gentle, patient, and truly Christian character.

Mr. Wilkins, Professor of Architecture to the Royal Academy, became a member of Caius College, Cambridge, in 1796, and took the degree of B.A. in 1800, his name standing sixth on the mathematical Tripos. He was soon afterwards nominated one of Wort's Travelling Bachelors, and also a fellow of his college, and passed four years in Greece and Italy, studying the architectural remains and monuments of those countries with great diligence, preparatory to the practice of his profession as an architect, which his father had followed with credit, and for which his great skill as a draftsman particularly qualified him. The study of those matchless creations of ancient art would appear to have exercised a powerful influence on his taste, and to have led him to prefer the purer forms of Grecian architecture to the more varied imitations and adaptations of them which appeared in the works of the Romans or in those of the great masters of modern Italy and more particularly of Palladio;—and the influence of these predilections was sufficiently visible in his designs for the East India College at Haileybury, and for Downing College, Cambridge, and is more or less easily traceable in most of his subsequent works. In 1807, he published his "Antiquities of Magna Græcia," a magnificent work, containing descriptions, views, measurements, and restorations of the chief remains of Syracuse, Agrigentum, Ægesta, and Pæstum. At a subsequent period he published "Atheniensia," or Remarks on the Buildings of Athens, in which he expressed opinions unfavourable to those commonly entertained respecting the rank which the Elgin marbles, which had been only recently purchased by the nation, should be considered to hold when viewed as works of art: he was likewise the author of a translation of the Civil Architecture of Vitruvius, including those books which relate to the public and private edifices of the Ancients, which was preceded by a learned introduction on the history of the Rise and Progress of Grecian Architecture,—a work which was chiefly designed to show that the precepts of Vitruvius referred to Grecian and not to Roman buildings.

The publication of these works and of some essays in the Archæologia, which showed a profound knowledge of the principles both of Grecian and Gothic architecture, led to very extensive

[* Some particulars of the life of Dr. Halley, collected by Prof. Rigaud, will be found in L. and E. Phil. Mag., vol. vi. p. 306; and other communications by him in vol. viii.—Edit.]

professional engagements, particularly in the University of Cambridge, where he rebuilt Corpus Christi and King's colleges, and made extensive additions to Trinity College: he was likewise the author of the magnificent portico of London University College, the National Gallery, and of other important edifices in London. He was latterly compelled by the declining state of his health and by repeated attacks of the gout, to retire from his professional engagements, though he did not abandon those studies which had formed his delight and occupation from his earliest years. In 1837, he published his "Prolusiones Architectonicæ, or Essays on subjects connected with Grecian and Roman Architecture," which were designed, in some degree, as a substitute for those lectures, which, under other circumstances, he would have been called upon to deliver, as Professor of Architecture, to the students of the Royal Academy. During the last year of his life, though constantly confined to his bed, and extremely weakened and emaciated by disease, he still continued his favourite pursuits until within a few days of his death, which took place on the last day of August last.

The Rev. Archibald Alison, senior Minister of St. Paul's Chapel, Edinburgh, was born in 1757, became a member of the University of Glasgow in 1772, and of Baliol College, Oxford, in 1775, and took the degree of B.C.L. in 1784: he soon afterwards took holy orders in the English Church, and was presented to several ecclesiastical preferments by Sir William Pulteney, Lord Chancellor Loughborough, and Bishop Douglas of Salisbury. In 1784 he married the daughter of the celebrated Dr. John Gregory of Edinburgh, with whom he lived in uninterrupted happiness for forty years of his life. His celebrated Essay "on the Nature and Principles of Taste" was first published in 1790, and speedily became incorporated into the standard literature of Great Britain. Towards the close of the last century, he became a permanent resident in his native city as minister of the Episcopal chapel, Cowgate, and afterwards of St. Paul's, where he was connected by congenial tastes and pursuits with Dugald Stewart, Playfair, Dr. Henry Mackenzie, Dr. Gregory, and the many other distinguished men who, during so many years, made that beautiful and picturesque city the metropolis of British literature. In 1814, he published two volumes of sermons; and at a later period, a very interesting memoir of his accomplished friend the Hon. Fraser Tytler Lord Woodhouslee. Mr. Alison was a man of very pleasing and refined manners, of great cheerfulness and equanimity of temper, of a clear and temperate judgment, and possessing a very extensive knowledge of mankind. He was habitually pious and humble-minded, exhibiting, in the whole tenor of his life, the blessed influence of that Gospel of which he was the ordained minister. All his writings are characterized by that pure and correct taste, the principles of which he had illustrated with so much elegance and beauty.

Edmund Law Lushington was born in 1766, at the lodge of St. Peter's College, Cambridge, of which his grandfather, Bishop Law, was master. He became a student, and afterwards a fellow of Queen's College in that University, and attained the fourth place on

the mathematical tripos in 1787. After practising for some years at the bar, he was appointed Chief Justice of Ceylon, a station which he filled for several years with great advantage to that colony. On his return from the East, he was made Auditor of the Exchequer, and also received from his uncle Lord Ellenborough the appointment of Master of the Crown Office. He was an intimate friend of Wollaston and Tennant; and though withdrawn by his pursuits from the active cultivation of science, he continued throughout his life to feel a deep interest in its progress. His acquaintance with classical and general literature was unusually extensive and varied, and he had the happiness of witnessing in his sons the successful cultivation of those studies which other and more absorbing duties had compelled him to abandon. Mr. Lushington was a man of a cheerful temper, of very courteous and pleasing manners, temperate and tolerant in all his opinions, and exemplary in the discharge both of his public and private duties: few persons have ever been more sincerely beloved either by their friends or by the members of their families.

Mr. George Saunders was formerly architect to the British Museum, where he built the Townley Gallery: he was also a diligent and learned antiquary, and the author of a very interesting and valuable paper in the twenty-sixth volume of the Archæologia, containing the results of an inquiry concerning the condition and extent of the city of Westminster at various periods of our history.

The only foreign members whom the Royal Society has lost during the last year are the Baron de Prony, one of the most distinguished engineers and mathematicians of the age; and the venerable Pierre Prevost, formerly Professor of Natural Philosophy in the University of Geneva.

Gaspard Clair Francois Marie Riche de Prony, was born in the department of the Rhone in 1755, and became a pupil, at an early age, of the Ecole des Ponts et Chaussées, where he pursued his mathematical and other studies with great application, and with more than common success. He was subequently employed, as an adjunct of M. Perronet, the chief of that school, in many important works, and particularly in the restoration of the Port of Dunkirk; and in 1786, he drew up the engineering plan for the erection of the Pont Louis XVI., and was employed in superintending its execution. M. de Prony had already appeared before the public, first as the translator of General Roy's "Account of the Methods employed for the Measurement of the Base on Hounslow Heath," which was the basis of the most considerable geodesical operation which had at that time been undertaken; and subsequently, as the author of an essay of considerable merit, "On the Construction of Indeterminate Equations of the Second Degree." In 1790 and 1797, appeared his great work, in two large volumes, entitled *Nouvelle Architecture Hydraulique*, which is a very complete and systematic treatise on Mechanics, Hydrostatics and Hydraulics, and more particularly on the principles of the steam-engine and hydraulical engineering. In 1792 he was appointed to superintend the execution

of the Cadastre, or great territorial and numerical survey of France,—a gigantic undertaking, the subsequent execution of which, during the revolutionary government, combined with the establishment of the bases of the decimal metrical system, gave employment and developement to so many and such important scientific labours and discoveries: among many other laborious duties, the formation of the extensive tables devolved upon M. de Prony, who, in the course of two years, organized and instructed a numerous body of calculators, and completed the immense *Tables du Cadastre,* which are still preserved in MSS. at the library of the Observatory in seventeen enormous folio volumes.

M. de Prony became Directeur-Général des Ponts et Chaussées in 1794, and was nominated the first Professor of Mechanics to the Ecole Polytechnique;—an appointment, which led to the publication of many very important memoirs on mechanical and hydraulical subjects, and on various problems of engineering, which appeared in the Journal of that celebrated school. He declined the invitation of Napoleon to become a member of the Institute of Egypt,—a refusal which was never entirely forgotten or pardoned. In the beginning of the present century he was engaged in the execution of very extensive works connected with the embankments towards the embouchure of the Po, and in the ports of Genoa, Ancona, Pola, Venice, and the Gulf of Spezzia; and in 1810, he was appointed, in conjunction with the celebrated Count Fossombroni of Florence, the head of the *Commissione de l'Agro Romano,* for the more effectual drainage and improvement of the Pontine Marshes. The result of his labours in this very important task, which he prosecuted with extraordinary zeal and success, was embodied in his *Déscription Hydrographique et Historique des Marais Pontins,* which appeared in 1822, which contains a very detailed description of the past, present and prospective condition of these pestilential regions, and a very elaborate scientific discussion of the general principles which should guide us, in this and all similar cases, in effecting their permanent restoration to healthiness and fertility.

After the return of the Bourbons, M. de Prony continued to be employed in various important works, and more particularly in the formation of some extensive embankments towards the mouth of the Rhone. In 1817, he was made a member of the *Bureau des Longitudes,* and in the following year he was elected one of the fifty foreign members of the Royal Society: in 1828, he was created a Baron by Charles X., and was made a peer of France in 1835. He died in great tranquillity at Aonières near Paris, in July last, in the 84th year of his age.

The Baron de Prony was a man of singularly pleasing manners, of very lively conversation, and of great evenness of temper. He was one of the most voluminous writers of his age, generally upon mathematical and other subjects connected with his professional pursuits; and though we should not be justified in placing him on the same level with some of the great men with whom he was associated for so many years of his life, yet he is one of those of whom

his country may justly be proud, whether we consider the extent and character of his scientific attainments, or the great variety of important practical and useful labours in which his life was spent.*

Pierre Prevost was born in 1751, and was originally destined to follow the profession of his father, who was one of the pastors of Geneva: at the age of twenty, however, he abandoned the study of theology for that of law, the steady pursuit of which, in time, gave way to his ardent passion for literature and philosophy: at the age of twenty-two, he became private tutor in a Dutch family, and afterwards accepted a similar situation in the family of M. Delessert, first at Lyons, and afterwards at Paris. It was in this latter city that he commenced the publication of his translation of Euripides, beginning with the tragedy of Orestes;—a work which made him advantageously known to some of the leading men in that great metropolis of literature, and led to his appointment, in 1780, to the professorship of philosophy in the college of Nobles, and also to a place in the Academy of Berlin, on the invitation of Frederick the Great. Being thus established in a position where the cultivation of literature and philosophy became as much a professional duty as the natural accomplishment of his own wishes and tastes, he commenced a life of more than ordinary literary activity and productiveness. In the course of the four years which he passed at Berlin, he published *Observations sur les méthodes employées pour enseigner la morale; sur la théorie des gains fortuits; sur le mouvement progressif du centre de gravité de tout le système solaire; sur l'origine des vitesses projectiles; sur l'économie des anciens gouvernements; sur l'état des finances d'Angleterre;* and he also completed the three first volumes of his translation of Euripides. There were, in fact, few departments of literature or philosophy which were not comprehended in the extensive range of his studies and publications.

In the year 1784, he returned to Geneva to attend the death-bed of his father, when he was induced to accept the chair of belles lettres in the University,—an appointment, which he found on trial little suited to his taste, and which he shortly afterwards resigned. For some years after this period, he was compelled more by circumstances than by inclination to partake largely in those political discussions, which, for some years, agitated his native city, and which afterwards, resumed upon a wider theatre, shook to its centre the whole framework of European society; but he gradually withdrew himself from political life on his appointment to the chair of natural philosophy in 1792, and devoted himself from thenceforth, with renewed activity and ardour, to pursuits which were most congenial to his tastes.

In 1790 M. Prevost published his *Mémoire sur l'équilibre du feu*, and in the following year his *Recherches sur la chaleur*: these important memoirs were followed by many others on the same subject

[* A paper by Prony, comparing the expansive force of the vapours of water and alcohol, appeared in Phil. Mag., First Series, vol. i. p. 345.—Edit.]

in various scientific journals; and the general results of all his researches and discoveries were exhibited, in a systematic form, in his well-known work *Sur le calorique rayonnant,* which was published in 1809, and in which he fully developed his *Theory of Exchanges,* and was enabled to give a consistent explanation of the principal facts which were at that time known respecting the nature and propagation of heat.

It would be impossible, in the very short compass within which this notice is necessarily confined, to enumerate even a small part of the publications of an author whose pursuits were so various and whose labours were so unremitting. He contributed papers to our Transactions in 1797 and 1803; the first containing an explanation of some optical experiments of Lord Brougham, and the second, some remarks on heat and on the action of bodies which intercept it, with reference to a paper by Dr. Herschel*; and in 1806, he became one of the foreign members of our body. In 1799, he obtained the first *accessit* for an essay *Sur l'influence des signes rélativement à la formation des idées,* which was written for a prize, adjudged to the celebrated Degerando, proposed by the Institute of France; and he was shortly afterwards elected a corresponding member of that body. His *Essais de philosophie, et études de l'esprit humain,* appeared in 1804, to which were appended some very remarkable Essays of his friend and ancient preceptor Le Sage, of whom he published a most interesting life in the following year. He likewise published, in very rapid succession, translations of the Rhetoric of Blair, the Essays and posthumous works of Adam Smith, the Elements of Philosophy of Dugald Stewart, the essay on Population by Malthus, Salt's Travels in Abyssinia, the Conversations on Political Economy, of his wife's sister-in-law, Mrs. Marcet, and many other works of less importance and interest.

In 1823, at the age of 72, though still vigorous and active both in body and mind, he resigned the professorship of natural philosophy, in wise anticipation of the approach of that period of life when men naturally feel reluctant to acknowledge the decline of their faculties, or incompetent to perceive it. From this time, though still consulted by his colleagues and fellow-citizens on every important subject connected with the Academy or the state, he retired into the bosom of his family, which contained within itself, in a very uncommon degree, every element of tranquillity, contentment and happiness. His own temper was singularly equable and tranquil; and his tastes and pursuits, which rarely left his time unoccupied, saved him from that *tædium vitæ* which sometimes renders old age querulous and discontented. Thus happily disposed and happily circumstanced, it is not wonderful that his life should have been prolonged beyond the ordinary limits of humanity. He died on the 8th of April, in the 88th year of his age, surrounded by his family, and deeply regretted by all who knew him.

The philosophical character of M. Prevost had been greatly influ-

[* See Phil. Mag., First Series, vol. xiii. p. 291.—Edit.]

enced by that of his master Le Sage, a man of great originality and profundity of thought, but whose speculations, particularly those which attempted the explanation of the cause of gravity, trespassed somewhat beyond the proper limits of philosophy. We consequently find him disposed to explain the laws of the propagation of heat and light on the most simple mechanical principles, and to trace their origin and progress much farther than the experiments or facts will properly warrant; thus giving to his conclusions, in many cases, a much more hypothetical character than would otherwise have attached to them. M. Prevost had little acquaintance with the more refined resources of modern analysis; and his researches on many important branches of experimental and philosophical inquiry were consequently limited to reasonings which could be carried on by the most simple algebraical, or geometrical processes. But notwithstanding the restrictions which were thus imposed on his progress, the range of his philosophical researches was unusually extensive and various, and his discoveries on heat must always be considered as constituting a most important epoch in a branch of science which has recently received so extraordinary a developement in the hands of Fourier, Forbes, Melloni, and other philosophers.

GEOLOGICAL SOCIETY.

[Continued from p. 411.]

May 22.—A paper was first read, "On the Wells found by digging and boring in the gravel and London clay in Essex, and on the geological phenomena disclosed by them," by Dr. Mitchell, F.G.S.

Essex consists chiefly of London clay, but that portion of the county which lies to the north-west of a line drawn from Harlow to Ballingdon Hill, near Sudbury, and the long ridge extending from Purfleet to East Tilbury, are composed of chalk. Extensive districts, however, are covered by thick deposits of gravel, sand, and other detritus, varying in depth from 10 to 300 feet. In Wakering Marshes and Foulness Island, there are 300 feet of sand between the vegetable soil and the London clay.

The wells formed in the gravel are supplied by land springs, the water, when enough, being collected in a reservoir excavated in the London clay. They are often not more than 12 feet in depth; but it is impossible to estimate the number of feet to which they must be sunk in any district, on account of the great inequalities of the outline of the chalk. At Stanway, near Colchester, the clay was found to be 45 feet from the surface; but at the Union workhouse, less than a quarter of a mile distant, and on the same level, it was necessary to sink 60 feet before it was reached. When the London clay forms the surface there are no land-springs, as the clay is generally impervious to water; but in some places it is sandy, and permits the percolation of water. That much of the rain which falls in Essex penetrates downwards, is evident from the smallness of the number and size of the brooks and rivers. Very little water enters the Lea on the west side; and into the Thames only four

streams flow between the Lea and Purfleet. There are three rivers, the Crouch, the Blackwater, and the Coln, but they are small, and can carry off only an inconsiderable portion of the water, which falls on about a million of statute acres.

The London clay in Essex is of great but variable thickness. It is seldom, however, that its actual dimensions can be ascertained, for though the depth of the wells is known, accurate details of that at which the clay commenced and terminated have not often been preserved. Dr. Mitchell gives the following list of the total depth of wells, selected from a very large number:—

Stratford	247	feet.
Ilford	301	—
Dagnam Hall	404½	—
Brook-street	340	—
Upminster	192	—
Parsonage, Warley	390	—
Grange Hill, near Fairlop	398	—
Dunton	344	—
Battle Bridge	350	—
Ferry-house on the Crouch	360	—
Rochford Union workhouse	330	—
Wakering Marshes	400	—
Foulness Island { sand...... 300; clay, 100 to 160 }	460	—
Clay-street, Walthamstow	190	—
Loughton, in Epping Forest	324	—
Epping	270	—
Horsley Park, near Ongar	340	—
Bocking	370	—
Braintree	420	—

This variation the author conceives, is partly due to the unevenness in the surface of the chalk; but in some instances to the undulatory nature of the country, the difference in the depth of the wells agreeing with the increase in the rise of the ground. When this is the case, the bed in which the water is found is the same in the adjacent wells, and consequently the variation in the outline of the surface is due to denudation, and not to unequal elevation. Thus, in the two wells close to the turnpike at Romford, water was found at the depth of 100 feet, but half-way up the hill between Hare-street and Havering Ate Bower at the depth of 250 feet; at Bocking it was obtained at 370, but at higher ground, at Braintree, close adjoining, at 420. Again, at the union workhouse in Rochford, the well is 330 feet deep, and at Stroud Green, on the road to Rugleigh, where the surface is higher, it was necessary to sink 390 feet. At North Fambridge is a well 388 feet deep, the water rising to within 10 feet of the top: but at another well in the same parish, dug in lower ground, there is a constantly flowing stream.

In the New River Company's well at the end of Tottenham Court Road, chalk was found at the depth of 150 feet; but in that near Pond-street, at Hampstead, the main spring in the bed of sand

between the London clay and the chalk, was 330 feet from the surface.

The London clay in Essex varies greatly in colour, being in some places yellow or red in the lower part, but in many localities it is blue to the bottom. It is sometimes uniform in composition throughout, but more frequently, even when only 100 feet in depth, divided into two or three portions by beds of sand. In the well at the site of Fairlop Fair it was 398 feet thick, and uniform throughout. In the Dengey and Rochford hundreds, where the clay is from 300 to 400 feet in thickness, it is divided by beds of sand into three or four parts. A bed of sand also usually occurs between the clay and the chalk. These alternations Dr. Mitchell is of opinion, indicate successive periods of turbulence and tranquillity.

A sufficient supply of water is sometimes obtained in the first bed of sand, but it is more often necessary to sink to that resting immediately on the chalk, on reaching which a vast volume of water rushes up, and compels the well-digger to ascend precipitately to the surface. Cement-stones are sources of great impediment, particularly to well-borers, a week or fortnight being occasionally spent in punching through a single mass. At the bottom of the clay a layer frequently occurs, and is technically called the water-rock, because, being penetrated, a powerful spring rushes up.

The water is sometimes, but not very often, combined with a saline substance, probably sulphate of magnesia, as that salt is abundant in the waters of the London clay in Surrey, and solid magnesia occurs at Stamford Hill, near London. Foul air is not unknown in the wells, though it has done little harm in Essex. Its nature has not been ascertained, but Dr. Mitchell conceives, that it is probably sulphuretted hydrogen, as in Middlesex and Hertfordshire that gas has been most destructive. In the chalk of Surrey carbonic acid gas is very troublesome, and has sometimes produced fatal effects.

There is, perhaps, no part of the world where artesian wells are more general, or are more useful than in Essex. In the vale of the Lea they have been bored with the greatest facility and at a small expense. In Waltham Abbey the cost is usually about 16*l*. In the district of Bulpham Fen, seven miles south from Brentwood, they yield a large supply of water. In the marshes, as well as along the coast, and in the islands of Essex, they have proved of the greatest utility. Formerly, in some seasons, when the ditches became dry, the cattle suffered, the fishes died, and the farmer lost severely on his stock; but by the aid of artesian wells the ditches are now kept full all the year, and the farmer and landlord are accordingly benefited. In Foulness Island there are no natural springs, and until lately no water, except atmospheric, collected in the ditches. In hot seasons this water became putrid, but the inhabitants and the cattle continued to partake of it as long as it lasted; and supplies were then obtained, at the distance of seven miles, from the east end of the island. Artesian wells now keep the ditches full of fresh and sweet water, labourers are obtained at reduced wages, and farmers

of a higher class are beginning to reside on the island. Wallisea, Mersea, and other islands have profited in a similar manner.

A great addition is made annually to the land along the coast of Essex, and valuable districts, one amounting to five hundred acres, and another to between one hundred and two hundred, have been recently protected by embankments. Outside of these inclosures are tracts of sand, estimated equal to 33,000 acres, not yet covered with vegetable mould, but dry eight hours out of every tide. Towards the close of 1837, preparatory steps were taken for forming a company to inclose these sands, but Dr. Mitchell is of opinion that they would not yield in 300 years a rental of 300 pence.

To this paper was appended a notice, by the same author, of constant and occasional outbursts of water from the chalk.

The localities of constant outbursts are, the Bourne Mill, near Farnham; the head of the river Mole, near the church at Merstham; (this river flows south of Ryegate to Dorking, below which town the bed of the river is dry in summer, but an abundant stream passes under the chalk, and reappears lower down;) Leatherhead, close to the Guildford road; the powerful spring near the church below Croydon; Orpington; the Holy-well at Kempering, on the south side of the North Downs; the spring a quarter of a mile west of Sittingbourne; Birchington, in the Isle of Thanet; the Lyddon Spout in the cliffs between Folkstone and Dover; the Holy-well, at the foot of the cliffs forming Beachy Head, one mile from Eastbourne; the spring which is the source of the Chadwell, and the main spring of the Amwell.

Occasional Outbursts.—The Bourne, near Birchwood House. During the last outburst, which was in the spring of 1837, the water flowed in great volume to Croydon, and continued to do so for six weeks. Later in the same year, another rivulet burst forth in Gatton Park, between Merstham and Ryegate; and a third in Nonsuch Park, near Ewell.

A communication was next read, entitled, "A notice on the discovery of the remains of Insects, and a new genus of Isopodous Crustacea belonging to the family Cymothoidæ in the Wealden Formation in the Vale of Wardour, Wilts," by the Rev. P. B. Brodie, F.G.S.

The quarry in which these fossils were found, is situated near the village of Dinton, about 12 miles west of Salisbury. Not having been worked for two years, its structure could not be clearly ascertained, but the following section may be considered as affording a near approximation to the order of the beds:—

1. Clay, forming the surface, a few inches.		
2. White limestone	3	inches.
3. Clay	2 to 3	—
4. White limestone, similar to No. 2, containing shells and cypris	3 to 4	—
5. Crystalline grit with cyclas	2	—
6. Clay	2	—

7. Clay, with layers of grit..........	3	inches.
8. Clay	2 to 3	—
9. Light brown sandstone, full of small cypris and cyclas, and consisting in the lower part of comminuted shells	18	—
10. Blue and lower clay, abounding with fragments of shells............	—	—
11. Thin-bedded grit	2	—
12. Fibrous carbonate of lime........ 13. Grit .... 14. Fibrous carbonate of lime........	6	—
15. Soft shelly sandstone............	2	—
16. Light brown and blue limestone, abounding with the Isopodous Crustacean; in the lower part, laminated and numerous cyclades, and a few small oysters........	6	—
17. Blue compact grit, full of impressions of cyclas.................	2 or 3	—
18. White laminated crystalline limestone, very different from that forming Nos. 2 and 4.		
Water—inferior strata not visible.		

The Isopods in the bed No. 16 often occur in clusters. Lenses of the eye are sometimes detectable in the limestone, and more rarely attached to the head; traces of legs have also been observed, but no antennæ. In the same bed the elytron of a coleopterous insect was discovered.

Among the heaps of debris, consisting of grits and limestones, derived apparently from beds subjacent to No. 18, but not visible, Mr. Brodie found fragments of a limestone different from the varieties in the preceding section, being generally coarser, softer, and less compact, and often white on the edges, but blue in the centre. It passes into a grit, in which he procured oysters, numerous bones and palates of fishes, and a tooth of a saurian. The limestone is full of a large distinct species of cypris; it contains also traces of carbonized wood, impressions of small plants, some of which resemble grasses; likewise remains of Isopods, a few bivalves, apparently cyclades, one fragment of a univalve, and, dispersed throughout its substance, insects and small fishes, sometimes microscopic. The insects discovered by the author consist chiefly of coleoptera, but he procured a beetle with the antennæ attached, about half an inch in length; remains of a Homopterous insect, and probably of several species of Dipterus, presenting distinctly, in some specimens, the wings, legs, and striæ of the abdomen; also a wing of a Libellula. Mr. Brodie believes that this is the first instance of the discovery of insects in a Wealden formation; and he observes, that for abundance and variety of specimens, the beds of the quarry resemble more a tertiary (Aix and Œningen) than a secondary deposit.

Mr. Brodie infers, from the occurrence of oysters in some of the layers, that the beds were accumulated in an estuary which afforded considerable variations in the nature of the sediment accumulated, and of the animals by which it was frequented.

In conclusion, the author states, that he is indebted to Mr. Owen for determining the characters of the fossil Isopod.

A letter was afterwards read, addressed to the Rev. Dr. Buckland, President of the Society, by R. Griffith, Esq., P.G.S. of Dublin, respecting the geological relations of the several rocks of the South of Ireland.

This communication was accompanied by a copy of Mr. Griffith's Geological Map of Ireland; and its principal object is to explain why he has coloured, as old red sandstone and carboniferous limestone, extensive districts of the counties of Kerry, Cork, and Waterford, which had been previously considered to be transition.

The geological base of these counties is clay slate passing into quartzose slate, quartz rock, and occasionally conglomerates. This is particularly the case in the peninsula of Corkaguinny or Dingle in the county of Kerry; and as the succession of rocks forming the south of Ireland is well exposed in that district within a short distance, Mr. Griffith selected it for the purpose of explaining his views.

The lowest formation on the sea-shore at Brandon Bay consists of black and red clay slate, and gray quartz rock. The beds are nearly vertical, but occasionally dip 70° or 80° to the south. In some localities near the Bay, the slates alternate with red and gray quartzose conglomerates; and on the western coast of the peninsula, at Doonguin, Ferriter's Cove, and Filaturrio, S.E. of Dingle, the slate contains Orthis, Terebratulæ, corals, &c. This series is succeeded, unconformably, by beds composed of rolled masses of quartz and mica slate, in an arenaceous base, and it is assigned by Mr. Griffith to the old red sandstone. On the summit of Cahirconree mountains, this conglomerate, associated with beds of fine-grained red sandstone, dips to the east at an angle of 10°. Proceeding eastward, in ascending order, the conglomerate disappears, and the formation consists of red and reddish-brown quartzose sandstone, alternating with coarse-red slate, flagstone, and occasionally green slates. These strata are succeeded, conformably, by a fine, yellowish-gray sandstone, forming the commencement of the carboniferous series. The sandstone contains Calamites, and at Gortaclay, 2 miles west of Curreen's Bridge, indistinct bivalves. Its upper beds alternate with coarse and fine dark-gray clay slate, abounding with Productæ, Spiriferæ, Terebratulæ, Encrinites, corals, and other fossils. Continuing to ascend in the series, beds of carboniferous limestone, containing the same organic remains, alternate with the fossiliferous slate; then appear strata of gray, fine-grained, indurated sandstone, alternating in the upper part with slate; next, a series of strata of limestone and greenish-clay slate, containing the same fossils; beyond which the slate gradually disappears, and the whole mass is composed of limestone. In the flat central space between Curreen's Bridge and Castle Island, are probably shale and limestone. Near Castle Island occurs the upper

limestone, abounding in nearly every known fossil of the carboniferous limestone of Ireland; and eastward of Castle Island is displayed, in conformable position, the millstone grit, the lower shales of which contain, in considerable quantity, Encrinites, Posidoniæ, Spiriferæ, Productæ, Ammonites, Orthocera, &c.

The change effected by Mr. Griffith in this district, consists in removing the dark-gray and greenish-gray fossiliferous slate at Curreen's Bridge from the transition series to the lower part of the carboniferous limestone system, in consequence of its resting conformably on the sandstone, and dipping regularly under the limestone, as well as on account of its fossils.

Mr. Griffith then describes a line of country between Mount Leinster, in the county of Wexford, and the sea-coast south of Cork. This district presents a succession of east and west valleys, in which flow the Suire, Blackwater, Bride, and Lea, with intermediate ridges, more or less elevated. The valleys are occupied by limestone, beneath which, in each instance, are, in descending order, the carboniferous slates, yellow sandstone, red slate, quartz rock, the conglomerate and subjacent greywacké, thus presenting the whole of the former section with the exception of the millstone grit.

Since the reading of his paper on this district at the Meeting of the British Association at Newcastle, Mr. Griffith has revisited the country, and found that his views of its structure, given in that paper, are perfectly correct; and during his examination he directed his attention more particularly to the limestones in the neighbourhood of Cork. A detailed section from French Furze, south of Currigoline to Middleton and Broomfield, intersecting the limestones of Cork Harbour, was exhibited and described in the paper. It displays the same succession of formations, namely, carboniferous limestone, carboniferous slate, yellow sandstone, red slate, and quartz rock.

To prove more particularly the correctness of his views, Mr. Griffith gives a minute account of the structure of the Monavollagh Mountains, in the county of Waterford. The base of these mountains consists of greywacké, covered unconformably by alternations of coarse-red or brownish conglomerates, coarse-red slate, and red quartzose slate. From Crotty's Rock the conglomerates are succeeded southward by alternations of coarse-red slate and quartz rock, the latter being interstratified, in descending towards the Blackwater, with beds of roofing slate, which occur only in the upper portions of the red slate series. On approaching the Blackwater, the clay slate is succeeded, conformably, by yellowish-white sandstone, and sandstone slate, containing casts of Calamites. These strata are again overlaid, conformably, by the greenish-gray imperfect clay slate, which alternates with the limestone of the valley of the Blackwater. The limestone of this valley is connected with that of the counties of Cork, Tipperary, &c., allowed by other geologists to belong to the carboniferous limestone of Ireland. The dip of the limestone strata in the valley of the Blackwater varies from 20° to 75°. Mr. Griffith then shows, that a similar though

reversed order of succession prevails south of the valley; but as the strata dip southward 80°, they apparently overlie the limestone, the deceptive character being due to the contortions of the formations.

It is not possible to follow the author throughout his details, but he shows, as before stated, that there is a regular sequence of formation throughout the country to Cork Harbour, the only variations being in the direction and amount of the dips due to undulations in the formations, and in the strata themselves. The localities described in greatest detail are the valleys of the Bride and the vicinity of Cork.

Mr. Griffith is of opinion, that the bands of carboniferous limestone in the valleys of the south of Ireland are only patches of a vast deposit which once covered the old red sandstone and transition districts.

The memoir was accompanied by an extensive collection of fossils illustrative of the different formations of the country, but more particularly of the Cork limestone. This collection was presented by Mr. Griffith to the Society.

June 5,—A paper was read, "On bones of Mammoths found in the deep sea of the English Channel and German Ocean," by Capt. J. B. Martin, Harbour-Master, Ramsgate, and communicated by Sir John Rennie, F.G.S.

The Ramsgate fishermen employed in trawling in the North Sea and English Channel, frequently bring up in their gear, fragments of fossil bones. These remains being generally charged with worms, and covered with fetid marine substances, are seldom capable of being preserved; but specimens in a good condition are sometimes procured, and of the greater part of these, Capt. Martin has been the fortunate purchaser. The following is a list of the principal specimens:—

1. A tusk, 9 feet long, and 8 inches in diameter at the lower end; but the part containing the alveolar cavity is wanting, and therefore its length or greatest diameter, when perfect, cannot be ascertained. The outside consists of very thin laminæ, and the interior of a soft substance resembling putty. The specimen was found in 1827, and is in the possession of Mr. Forster of Ramsgate.

2. In 1835, a very large decayed bone, and a tusk 11 feet long, but so soft as to be cut through with a knife, the centre being of the consistence of pipe-clay, were dredged up between Boulogne and Dungeness. The bottom of the channel, at that point, consists of blue clay charged with rounded pebbles.

3. In 1837, a fisherman, trawling between the two shoals called Varn and Ridge, and in 21-fathom water, enclosed in his net a vast mass of bones, but of which only a humerus was preserved. The upper articulation is wanting, but the length of the portion obtained is 38 inches; the circumference of the upper part of the shaft, 31 inches; of the centre, 20 inches; of the part just above the condyle, 31 inches: and the width of the condyle is

10 inches. The Varn and Ridge lie in the mid-sea between Dover and Calais, forming a line of submarine chalk hills, which trend towards the north, and are parallel with the cliffs on the opposite sides of the Channel. The Overfalls and Galloper Sands, continuations of the same line, are also steep, having deep gullies in their intermediate spaces filled with boulders and muddy ground.

4. A tusk, 78 inches long and 12 inches in circumference, but the part containing the alveolar cavity is wanting. Its curvature is equal to a semi-circle, turning out. It was trawled up at the back of the Goodwin Sands. Capt. Martin has also a fragment of a fossil tree from the same locality.

5. In the early part of 1839, a nearly perfect femur of a mammoth was obtained about midway between Yarmouth and the coast of Holland, in 25 or 26 fathoms, low-water. The length of this femur, from the ball of the socket-joint to the lower condyle, is 49 inches; the circumference of the ball, 24 inches; of the upper part of the shaft, 42 inches; of the centre, 18 inches; of the lower part above the condyle, 29 inches.

6. Two molars of the mammoth brought up in the gear of the fishermen, in different parts of the English Channel, and likewise in Capt. Martin's cabinet.

Mr. Fairholm of Ramsgate has also in his possession a molar of a mammoth, found in King-street of that town, in red clay resting upon chalk.

Independently of the remains of mammalia, the fishermen are occasionally impeded in their operations by large masses of various descriptions of rock. Some of these blocks are much worn and rounded; but the remainder never present that irregularity of form which might lead to the supposition, that they had composed part of shipwrecked cargoes.

With respect to the distribution of the animal remains and the boulders, Capt. Martin states, that they are never found on the summits of the banks or shoals, but in deep hollows or marine valleys; and that they thus agree, in position, with analogous remains and masses of rock found upon dry land.

An extract from a letter addressed to Dr. Buckland by Sir John Trevelyan, Bart., was then read.

That gentleman possesses a very large molar of an elephant, found 38 years ago in the bed of the Severn near Watchet. He also states, that Roman pottery has been frequently dredged up during the last 50 years from the estuary of the Thames near Margate; that there is an island off Herne Bay, called Pot Island, on account of the quantity of earthenware found near it. A Roman vessel, laden with pottery, is supposed to have been wrecked in the neighbourhood of this spot.

A paper entitled, "Description of five Fossil Trees found in the excavations for the Manchester and Bolton Railway," by John Hawkshaw, Esq., F.G.S., was next read.

The largest of these trees was discovered about two years since, and the other four during the spring of the present year (1839), in

that portion of the Lancashire coal-field intersected by the railway. They are all in a vertical position with respect to the plane of the bed, which dips about 15° to the south; and they stand in a straight line, though obliquely to the strike of the strata. The distance between the first and the last is about 100 feet, but the intermediate trees are not equally distributed. The roots are imbedded in a soft argillaceous shale; and in the same plane with them is a bed of coal 8 or 10 inches thick, which has been ascertained to extend across the railway, or to the distance of at least 10 yards. Just above the covering of the roots, yet beneath the coal-seam, so large a quantity of *Lepidostrobus variabilis* was discovered enclosed in nodules of hard clay, that more than a bushel was collected from the small openings around the base of the trees. The trunks were wholly enveloped by a coating of friable coal, varying from $\frac{1}{4}$ to $\frac{3}{4}$ of an inch in thickness; but it crumbled away on removing the matrix. The internal casts of the trees consist of shale traversed beneath the place of the bark by irregular longitudinal flutings less than $\frac{1}{4}$ of an inch broad, and about 2 inches apart. These markings, however, are stated to be very irregular. Mr. Hawkshaw also mentions indications of a waving, irregular, fibrous structure. The dimensions of the trees are as follows:—

	Circumference.	Height.
No. 1,	$15\frac{1}{2}$ feet at the base, $7\frac{1}{2}$ feet at the top......	11 feet.
No. 2,	9 —	$2\frac{1}{2}$ —
No. 3,	6 —	3 —
No. 4,	6 —	5 —
No. 5,	$7\frac{1}{2}$ —	6 —

No. 2 has three large spreading roots, nearly 4 feet in circumference; and they separate 5 or 6 feet from the trunk into 8 branches. The roots of Nos. 3 and 4 extend apparently but a short distance; those of No. 5, as far as exposed, are five in number, 4 feet in circumference, solid and strong, and are presumed to extend to a considerable distance. The position of No. 1 prevents its roots from being exposed.

Respecting the genus to which the fossils belonged, no positive opinion is offered.

The paper concludes with some observations on the disputed question, whether the plants associated with coal, grew on the spots where they have been found. Mr. Hawkshaw admits, that the vertical position of trees does not prove that they had not been drifted: but he conceives, from the experience which a residence in South America has afforded him, that it is more difficult to suppose that five drifted trees could be deposited erect in one spot, than that they grew where they occur.

Mr. Hawkshaw has not only prevented the trees from being removed, but he has had them protected, as far as possible, from the action of the weather.

A paper was then read, entitled "A notice of some Organic Remains recently discovered in the London Clay," by Nathaniel Wetherell, Esq., F.G.S.

The fossils described in this communication, were found about three years since in the excavations on the line of the Birmingham Railway, between Euston Square and Kilburn. They occurred at depths varying from 12 to 40 feet, and generally in small hard nodular masses of a pale-brown colour. Some of the specimens, when cleared from the matrix, are oval or spindle-shaped; others are cylindrical and branched, varying in diameter from half an inch to less than a tenth, and in length from 2 to 5 inches; and several are flabelliform, with a more or less rugose surface, the width of the largest being 4 inches and three quarters, the length about 5 inches, and the thickness half an inch. The whole of the specimens are more or less covered with small oviform grains, occasionally furrowed down the middle, and generally distributed without any definite arrangement, but in some instances are disposed in rows, the grains being chiefly placed parallel to their longer axis. Besides the above more regular-shaped masses, Mr. Wetherell has obtained a vast quantity of others, which present no definite form, but are composed of small rough angular bodies, generally amorphous internally, but occasionally composed of concentric lamellæ. These specimens are likewise often more or less covered with the oviform grains, some of which may also be discovered in the substance of the specimen. The author referred to a description by Mr. Richardson, of branched bodies, in the London clay near Herne Bay, but which are not covered by the oviform grains*.

Mr. Wetherell offers no opinion relative to the true nature of these fossils, leaving their determination open to the result of future researches.

Lastly, a paper was read "On the relations of the different parts of the Old Red Sandstone, in which organic remains have recently been discovered, in the counties of Murray, Nairn, Banff, and Inverness," by J. G. Malcolmson, M.D., F.G.S.

The author commences by stating, that in a paper read before this Society in April 1838†, he announced, that Mr. Martin had discovered fossil scales and bones in the old red sandstone under the cornstone four miles to the south of Elgin, and that he had himself ascertained that many of the specimens belonged to fishes from Clashbinnie, since figured in Mr. Murchison's Silurian System‡ under the name of *Holoptychus Nobilissimus*. A careful examination of the Ichthyolite beds discovered by Mr. Miller on both sides of the south Sutor of Cromarty, convinced the author, that they also belong to the old red sandstone; and he has identified several of the fishes found there with those of Gamrie, Caithness, and Orkney; and this identification M. Agassiz confirmed with reference to the Cromarty species of Cheiracanthus, Diplopterus, and the remarkable fossil called by that naturalist Coccosteus; the Gamrie species of Acanthodes, M. Agassiz likewise recognised

* Geol. Proceedings, vol. ii., p. 78. [or Lond. and Edinb, Phil. Mag., vol. v. p. 219.]

† *Ibid.*, vol. ii. [or Lond. and Edinb. Phil. Mag., vol. xiii. p. 226.]

‡ Plate, 2 *bis*.

among the Cromarty specimens. Mr. Murchison has given further proof of the age of the Caithness beds by showing that the *Dipterus macrolepidotus* so common in them, is found also in the tilestone or lowest member of the old red sandstone of England.

Dr. Malcolmson then proceeds to describe the discoveries recently made by himself, the Rev. G. Gordon, and Mr. Staples, of fossil fishes in a district of old red sandstone, extending from the village of Buckie, near Cullen, to Culloden Moor, 6 miles south of Inverness. The southern parts of this tract are occupied by primary rocks, which send off spurs and transverse ridges into the sandstone country, and they are likewise exposed in different places within its area. Wherever the contact of the two classes of rocks is exhibited, the old red sandstone rests on the edges of the older formations, dipping 8° or 12° a little west of north. The granite series also terminate at the junction with the sandstones. The old red sandstone Dr. Malcolmson divides into three portions, the lowest of which he calls the *Inferior* or *Great Conglomerate*; the middle, the *Central* or *Cornstone* division; and the uppermost, the *Fine Grain Sandstone* and *Quartzose Conglomerate*.

The lowest division is shown to belong to the great conglomerate at the base of the old red sandstone of Sutherland and Ross. The beds of which it consists are exposed in ravines on the right bank of the Nairn to the east of Inverness, also in the ravines above Cawdor Castle; but at Rait Castle they thin out, or were denudated, according to the author's view, before the deposition of the upper beds. On the east side of the hill of Rait they reappear, and extend along the Burn of Lethen for several miles. They occur also at Binnie in the vale of Rothes, south of Elgin, and along the Spey. The division consists of partially-rounded fragments of the primary rocks of the neighbourhood, cemented by a calcareous and ferruginous sandstone.

The Cornstone division consists of sandstones, calciferous concretions, conglomerates, and marls, and contains scales of the *Holoptychus Nobilissimus* and other fishes; also teeth and ichthyodorulites of new genera. This fossiliferous rock is exposed for a short distance at Scot-craig near Elgin, resting on the great conglomerate, and it passes below the cornstone of Elgin.

Resting on the Elgin cornstones is a series of very beautiful white and yellow siliceous sandstones, containing pebbles of quartz, gneiss, and granite. It may be traced from Quarry Hill near Elgin, to Burge, 3½ miles east of Forres, extending over a considerable part of the north-eastern district of Murray.

Dr. Malcolmson next describes, in detail, the cornstone series as it is displayed on the banks of the Findhorn, particularly where it is exposed between the gneiss and the Cothall limestone*, various remains of fish having been found there; and at Altyn, where he obtained scales of *Holoptychus Nobilissimus*, and abundance of

* Dr. Malcolmson refers to Prof. Sedgwick and Mr. Murchison's paper on this district for other information respecting the Cothall limestone, Geol. Trans., 2nd series., vol. III., p. 151.

Ichthyolites identical with those at Scot-craig near Elgin; also a section through the middle and inferior sandstones on the Burn of Lethen. Along this burn, from Earlsmill to Cald Hame, fine sections of sandstones, calciferous conglomerates, and marls similar to those of the Findhorn beds, are laid open, and the same organic remains are found in considerable numbers, with the addition of buckler-shaped bones allied to Cephalaspis. These beds rest at Cald Hame on a deposit of thin-bedded red sandstones and hard conglomerates, which are succeeded by a considerable thickness of hæmatitic red schistose sandstone, resting apparently on the Clunes limestone, containing Ichthyolites. These slaty beds resemble the upper red sandstones of Cromarty and Ross. In a small quarry in the grounds of Lethen, thin seams of shale and clay dip under the red sandstones, and contain nodules resembling those of Gamrie, and bituminous layers and remains of the species of Cheiracanthus common at Clunes; also plants resembling Fuci. Beneath the shales are a few feet of soft white sandstone, succeeded by the great inferior conglomerate.

The finest fish, often of a plum-blue colour, have been obtained from an excavation on the farm of Lethen-bar, in large nodules enclosed in a soft, reddish-brown schist, probably a prolongation of the shales. At Clunes, a mile to the eastward, similar remains occur in a stratum of clay and decomposed shale. The author has ascertained, by careful comparisons, that the known species obtained at the above localities are the same as those found in Orkney, Caithness, Cromarty, and Gamrie, and belong to the genera Dipterus, Diplopterus, Cheiracanthus, Cheirolepis, Osteolepis, Coccosteus, and another singular creature, which he proposes to describe hereafter. The plants above noticed, and fish scales, are also found near the hill of Rait in a ridge of red schistose sandstone.

The fossils of the valley of the Nairn are then described. Fragments and casts of tuberculated scales and bones, resembling some of those of Lethen-bar, occur at Balfreish in a compact light-blue limestone, containing angular fragments of gneiss, porphyry, &c., and an overlying conglomerate. At the S.E. extremity of Culloden Moor, and opposite the Druidical temples of Clova, are beds of bituminous shale, and a black calcareous rock, similar to the Caithness pavement, some of which contain nodules, often very small, enclosing fish scales and vegetable impressions. The bituminous rock, Dr. Malcolmson is of opinion, is continuous with that at Inches, 4 miles to the west, and 2 south-west of Inverness, described by Prof. Sedgwick and Mr. Murchison, and shown by them to be a prolongation of the bituminous schists of Caithness and Strathpeffer.

The banks of the Spey, the Burn of Tynat, and the strata at Buckie in Banffshire, have been discovered by Dr. Malcolmson and the Rev. G. Gordon, to contain the same remains. The localities mentioned are the beds of shale and red sandstone opposite Dipple, where remains of the Coccosteus, Dipterus, and *Osteolepis magus*, occur. These beds are overlaid by others resembling those which cover the Ichthyolites of Lethen and Cromarty.

Following the strike of the Dipple beds into Banffshire, the author and the Rev. G. Gordon discovered, at the Burn of Tynas, 4 miles E. of Fochabers, a similar series of shales and sandstones containing Ichthyolites, enclosed, as usual, in flattened nodules. Many fine specimens of species common to Lethen, Cromarty, &c., were procured in the highest stratum. At Buckie, the inferior conglomerate is partially covered by patches of a red schistose sandstone, in which a tuberculated bone, similar to those in the Burn of Tynat, was found. The shore near this point is said to exhibit fine examples of a raised beach.

From the facts contained in the paper, the author concludes,

1. The primary strata were thrown into highly-inclined positions before the deposition of the old red sandstone. The elevation of the secondary strata to their present position, he conceives may have been produced by elevation in the line of the Grampians, or of the Great Caledonian Canal, subsequent to the accumulation of the Purbeck beds at Linksfield.

2. The great conglomerate and red sandstones containing Dipteri, Cheiracanthi, &c., represent the Orkney, Caithness, and Gamrie strata in Scotland, and the inferior beds of the old red sandstone of England.

3. The superimposed marly conglomerates, sandstones, and marlstones, with a distinct series of fossils, are equivalents of the central division of the old red sandstone system to the south of the Grampians, and in England.

Lastly, That there are no indications of the coal strata.

Dr. Malcolmson terminates his memoir by stating, that the Gamrie Ichthyolites clearly belong to the old red sandstone, and not to the coal measures.

This being the last evening of the Session, the Society adjourned, at the conclusion of its business, to Wednesday, the 6th of November, 1839.

ZOOLOGICAL SOCIETY.

June 11, 1839.—Mr. Bucknell exhibited his *Eccaleobion*, or machine for hatching eggs; and having broken eggs in every stage of incubation, explained the nature and incidents of the process. Mr. Bucknell stated that the period of incubation in the common fowl, which was, on an average, 21 days, sometimes varied from 18 to 24 days, and that he attributed this variation to the mode of keeping, and previous treatment, by which the embryo was injured, either from the heat of the weather, exposure to variety of temperature, jolting in carriage, &c. The young bird was occasionally known to emit a faint chirp even so long as 24 hours before being excluded; and he believed that if this noise was heard on the 18th day the chicken would probably appear on the 19th. From this and other circumstances, such as the common mode of preparing eggs by varnishing, &c., the porosity of the shell, and other similar causes, he concluded that the small globule of air constantly found in eggs, and which he had observed to increase according to the age of the egg, was produced by the air penetrating the substance of the shell and its lining membrane.

The average number of malformations, according to Mr. Bucknell's experience, was not more than five in a thousand; though in Egypt, it was stated, malformations were extremely common in the artificial process of incubation. He attributed this circumstance to an excess of heat, and generally found it to affect the toes and extremities; sometimes also the muscles of the neck.

A general conversation afterwards took place on this subject, during which much interesting and valuable information was extracted, with regard to the period and circumstances of the incubation.

June 25.—Mr. Waterhouse read a paper on a new species of Rodent which had been sent from the island of Luzon, one of the Philippines, by Hugh Cuming, Esq., Corresponding Member.

In general appearance this Rodent might be mistaken for a species of *Capromys*; in size it is about equal to the *C. Fournieri*; the general characters of the skull and dentition, however, indicate that its affinity is with the *Muridæ*.

"The skull, compared with that of the common Rat, differs in being of a more ovate form, the occipital portion being somewhat elongated, and considerably contracted; the width between the orbits is comparatively great; and behind the orbits the frontal bones are expanded, and join with the temporal to form a distinct post-orbital process. The interparietal bone, instead of being transverse, is almost circular. The auditory bullæ are very small. The interdental portion of the palate is slightly contracted in front, so that the molares diverge posteriorly; the rami of the lower jaw are less deeply emarginated behind, the coronoid portion is smaller, and the descending ramus is broader and rounded; the symphysis menti is of considerable extent. The incisor teeth are less compressed and less deep from front to back. The molar teeth are of a more simple structure; the anterior molar of the upper jaw consists of three transverse lobes, and the second and third consist each of two transverse lobes. In the lower jaw the anterior molar consists of four lobes, a small rounded lobe in front, followed by two transverse lobes, of which the anterior one is the smaller, and finally a small transverse posterior lobe; the second molar consists of two equal transverse lobes, and a small lobe behind them; the last molar consists of two simple transverse lobes."

On account of the differences observable in the structure of the teeth, and form of the skull, combined with the hairy nature of the tail and ears, Mr. Waterhouse regarded this animal as constituting a sub-genus, and proposed for it the name of *Phlœomys*[1], this name being suggested by the habit of the animal, which Mr. Cuming (after whom the species is named) states, feeds chiefly on the bark of trees. It may be thus characterized:

MUS (PHLŒOMYS) CUMINGI. *M. vellere setoso, suberecto, pilis lanuginosis intermixtis; auribus mediocribus extùs pilis longis obsitis; mystacibus crebris et perlongis; pedibus permagnis et latis, subtùs nudis; caudâ mediocri, pilis rigidis et longis (ad*

[1] Φλοιὸς, bark (φλοίω, to decorticate), and Μῦς.

Murem Rattum *ratione habitâ) crebrè obsitâ : colore nigrescenti-fusco sordidè flavo lavato, subtùs pallidiore ; caudâ nigrescente ; pilis longioribus in capite et dorso nigris.*

	unc.	lin.
Longitudo *capitis corporisque*	19	0
——— *caudæ*..................	13	0
——— *antepedis (unguibus exclusis)*	1	8½
——— *tarsi*	2	10
——— *auris*	1	0
——— *cranii ossei*	2	4
Latitudo *ejusdem*	1	8½

Hab. apud insulam Luzon.

Some notes on the birth of the Giraffe at the Society's Menagerie were communicated by Professor Owen.

Of this paper the following is an abstract.

Connexion took place between the female Giraffe and the lighter-coloured male on the 18th March, 1838, and again on the 1st of April.

The young animal was a male, and was born June 9, 1839, being 444 days, or fifteen lunar months, three weeks, and three days, since the last observed, and, in all probability, the last coitus.

The new-born animal came into the world, like other Ruminants, with the eyes open, and the hoofs disproportionately large. The skin was marked as distinctly as in the adult, with large angular spots, which were somewhat darker than those of the mother; and the hair of the legs was of a deeper fawn colour. It sucked some warm cow's-milk from a bottle with avidity, and once or twice uttered a low, gentle grunt or bleat, something between that of a fawn and a calf. The young creature made several efforts to stand, raising itself on the fore knees; and was able to support itself on its vacillating and outstretched legs, about two hours after its birth.

"No one could have seen the young Giraffe," says Professor Owen, "without being struck with its large size, compact figure, and strength of limb. The condition or purpose of the long gestation is, evidently, to bring into the world the young Giraffe of a stature and strength suitable to the exigencies of a denizen of the desert—the birthplace, likewise, of the Lion and other destructives." The length of the animal, measuring from the muzzle to the root of the tail, was six feet ten inches; the girth of the trunk was two feet nine inches; from the tuber ischii to the patella was one foot four inches; from the patella to the apex of the hind hoof three feet; from the olecranon to the carpus was one foot ten inches; from the carpus to the end of the fore hoof was one foot eleven inches. These segments of the fore leg were thus nine inches longer than the corresponding ones of the hind leg; and as this disproportion does not exist in the adult, it offers another instance of the precocious developement of the anterior extremities in the mammiferous fœtus.

She would not yield her milk to, or even suffer her offspring to come near her. The young Giraffe was nourished by warm cow's milk. It gamboled actively about when one day old, and continued,

without appearance of illness, till the 28th of June, when it was attacked by convulsions, and died.

A paper was read "On the polarizing property of living animals and animal substances upon the rays of transmitted light," by Mr. J. F. Goddard*.

In this communication Mr. Goddard first alluded to the double refracting (polarizing property) exhibited by the lenses of the eyes of fishes and several other animal substances, an account of which was published in 1816 by Sir D. Brewster.

On repeating these experiments, Mr. Goddard, after observing that the scarf skin of the human subject, sections of human teeth, the finger nails, bones of fishes, and other substances, possessed the same property, was led to examine some living objects, when he discovered that among others, the larvæ and pupæ of a gnat (*Corethra plumicornis*) possessed this property in an eminent degree. The extraordinary transparency of this little insect is such, that the whole of its internal structure is beautifully displayed, and when viewed by polarized light, presents the most splendid appearance; the peculiar interlacing of the muscles of the body, dividing it into regular parts, present (as the insect changes its position with regard to the plane of polarization) the most varied hues and brilliant colours. Mr. Goddard stated that the same phænomena may also be seen, if possible, in a more splendid manner, in the spawn of many large fishes, which, in their early state are sometimes equally transparent, particularly those species which inhabit the sea.

The polarizing property of various substances was beautifully exhibited by Mr. Goddard, by means of his "Polariscope."

July 23.—The following paper, on the production of Isinglass from Indian Fishes, by Dr. Cantor, Corresponding Member, was read:

"In the December Number, 1838, of Parbury's Oriental Herald appears a letter 'On the Suleah Fish of Bengal, and the Isinglass it affords': the description of this fish I shall quote in the words of the anonymous writer. 'The Suleah Fish,' he observes, 'when at its full size, runs about four feet in length, and is *squaliform*, resembling the Shark species in appearance, but exhibiting a more delicate structure than the latter. The meat of this fish is exceedingly coarse, and is converted by the natives, when salted and spiced, into "burtah," a piquant relish, well known at the breakfast-tables of Bengal. The bladder of the *Suleah* may be considered the most valuable part of it, which, when exposed to the sun and suffered to dry, becomes purely pellucid, and so hard that it will repel the edge of a sharp knife when applied to it. These bladders vary from half a pound to three quarters of a pound avoirdupois in weight, when perfectly dry. . . . The *Suleah* Fish abounds in Channel Creek, off Saugor, and in the ostia or mouths of all the rivers which intersect the Sunderbuns, and are exceedingly plentiful at certain seasons.'

"Conceiving the great importance of the discovery of isinglass being a product of India, I was naturally anxious to examine the

* See the present volume, p. 152.

source, arising from a branch of natural history to which in particular I have devoted my attention; but from the general nature of the description, I was obliged to defer my desire of identifying the fish till some future opportunity should enable me to do so. Quite unexpectedly, however, a few days ago, the last overland despatch brought me a letter from my valued friend Mr. McClelland, a Corresponding Member of this Society, an extract of which, bearing upon the point in question, I lose no time in laying before the Society:—'. . . . I have now to mention what is of far greater importance in another point of view, namely, that the Suleah Fish described in a recent number of Parbury's Oriental Herald is the *Polynemus Sele* of Hamilton. I have examined that species, and found an individual of two pounds weight to yield sixty-five grains of pure isinglass, an article which here sells at sixteen rupees (1*l.* 12*s.*) per ℔. Refer to your dissections of *Polynemi*; mark those with large air-vessels to be isinglass, requiring no other preparation than merely removing the vascular membrane that covers them, washing with lime-water, and drying in the sun. You know the size these fishes attain, and the number in which they abound in the Sunderbuns; you also know the method of taking them, and can therefore state to what extent isinglass may be obtained in India. I have sent a paper on the subject to the Journal of the Asiatic Society, which I will send you by the next overland despatch.'

"Perceiving by this that the subject has been taken up by a naturalist of Mr. McClelland's rank, and that we ere long may expect his observations embodied in a paper from his hand, I think it sufficient to confine myself to a few general remarks upon those species of *Polynemus* which have come under my actual examination while I was attached as surgeon to the Hon. Company's survey of the sea-face of the Gangetic Delta.

"The species best known is the *Polynemus risua,* Hamilton; *Pol. longifilis,* Cuvier; the Tupsee or Mango Fish of the Anglo-Indians; this inhabits the Bay of Bengal and the estuaries of the Ganges, but enters the mouths of the rivers, even higher up than Calcutta, during the breeding-season (April and May), when the fish is considered in its highest perfection, and is greedily sought as a great delicacy. This species is the smallest, for its length seldom exceeds eight or nine inches, and one and a half to two inches in depth. *Polynemus aureus* and *Topsui,* Hamilton, are species closely allied to this.

"*Polynemus sele,* Hamilton, *P. plebeius,* Broussonnais, *P. lineatus,* Lacépède, is the Suleah Fish mentioned in Parbury's Oriental Herald, the same which Mr. McClelland submitted to examination. This species, as well as another closely allied to *P. quadrifilis,* Cuvier, which I have dissected, figured, and described, under the name of *P. Salliah* (*Saccolih*), appears equally plentiful, in shoals, all the year round in the estuaries of the Ganges, and is appreciated by Europeans and natives for its excellent flavour. Both species attain a size from three to four feet in length, and eight to ten inches in depth.

" In a paper which I had the honour of communicating to the Royal Asiatic Society*, the genus *Polynemus*, among others, was pointed out by me as forming an article of food fit for curing, and easily procurable in almost any quantity: by the discovery that it produces isinglass, it has attained an additional interest; and I have no doubt the manufacture of this article will, when entrusted to judicious hands, form another valuable article of exportation from India."

MICROSCOPICAL SOCIETY.

At a meeting held at the house of E. J. Quekett, Esq., Wellclose-square, Sept. 3rd, 1839, to take into consideration the propriety of forming a Society for the promotion of Microscopical Investigation, and for the introduction and improvement of the Microscope, as a scientific instrument;—

Present, the following Gentlemen:

Rev. J. T. Bean, Mr. J. S. Bowerbank, Dr. F. Farre, Mr. W. Francis, Mr. Greening, Mr. Jackson, Mr. Lister, Mr. G. Loddiges, Mr. C. Loddiges, Mr. E. J. Quekett, Rev. J. B. Reade, Mr. Rippingham, Mr. Ross, Mr. R. H. Solly, Mr. C. Varly, Mr. N. B. Ward, Mr. A. White.

It was "Resolved, that such a Society should be formed; that a provisional Committee be appointed to carry the resolution into effect; and that the said Committee do consist of the following gentlemen;—

Messrs. Bowerbank, Lister, Loddiges, Quekett, Reade, Solly, and Ward."

The Provisional Committee having prepared an outline of a Constitution for the Society, a Meeting was advertised to be held at the Horticultural Society's Rooms, on the 20th Dec. 1839 for the further consideration of the subject. The meeting was numerously attended; Professor Owen, F.R.S., &c. took the chair and was elected President, after which the Treasurer, N. B. Ward, Esq., the Secretary, Dr. A. Farre, and the Council were appointed; the constitution prepared by the provisional committee was unanimously adopted by the meeting, and the President announced that the future meetings of the society would be held in the Horticultural Society's Rooms. The Society will be designated the Microscopical Society; its objects are to promote improvements in the Optical and Mechanical construction of Microscopes; the reading and discussion of papers upon new and interesting subjects of microscopical inquiry; the formation of a collection of rare and valuable microscopical objects, and of a library of reference.

At the close of the business of the evening upwards of fifty gentlemen joined the society. The terms upon which members are admitted are unusually light, being one guinea entrance and a yearly subscription of one guinea.

* Published in the Journal of the Royal Asiatic Society of Great Britain and Ireland, No. ix., August 1838, p. 165.

THE ITALIAN SCIENTIFIC ASSOCIATION: MEETING AT PISA, OCTOBER 1ST to 15TH, 1839.

All the members of the Italian Scientific Association are admitted to the privileges of membership without any pecuniary payment. An open field is granted to them in the sectional meetings of the Association for the communication of their discoveries and observations; constant opportunities are afforded for the encouragement of conversation, and a tribute of respect is paid to a large portion of the members, by the formation of a select society, within the Association, consisting of those individuals who have worked for the advancement of science, and including, at the same time, the members of distinguished scientific societies.

Great praise is due to Prince Charles Bonaparte, aided by several eminent Professors of Pisa and Florence, for their exertions in originating the Italian Scientific Association. These philosophers were fortunate in obtaining the zealous cooperation of Leopold II., the Grand Duke of Tuscany, in the preliminary arrangements for the meeting; and the attendance of that sovereign at the sectional meetings of the Association, and his willing attention bestowed on the scientific papers which were read in the various sections, plainly demonstrated the interest which he felt in the proceedings of this society.

On the 1st October, 1839, a religious service was performed in the cathedral at Pisa, to which the Catholic members of the Association were invited. After this service, an introductory meeting of the scientific members was held in the university. On the following day, a beautiful marble statue of Galileo, which had been recently erected in the centre of the court of the university, was exhibited to the members of the Association, and a formal oration was delivered by Professor Rosini of Pisa in honour of Galileo.

The first general meeting of the Italian Association took place on the 3rd of October, and Professor Gerbi, the aged President of the Association, read a long address on this occasion, in which he expatiated largely on the praises of Galileo, and detailed some of the principal advantages of the six sections of the Association. Several letters from the absent friends of the Association were also read at the same meeting, and among these letters, one of the most interesting was from Sir John Herschel.

Sectional meetings commenced on the 4th October; early hours were adopted, and the sections of agriculture and zoology commenced their labours at eight o'clock in the morning. Only two hours were allowed for each sectional meeting, and at ten o'clock the first two sections concluded their business for the day, and the sections of geology and chemistry were allowed to commence their sittings, which ended at twelve o'clock.

Between twelve o'clock and two, the sections of medicine and botany were at work, but the popularity of the medical section, and the large number of contributors to its stores, often rendered a longer period than two hours requisite for the discussion of the various subjects brought before its notice.

All the sections were well attended, and the audience appeared interested, and at times greatly delighted, especially during the most animated discussions.

In the geological section, and probably in all the other sections, the results of long and laborious investigations were brought forward by their authors. Professor Sismondi of Turin, the president of the geological section, exhibited to the members of that section his accurate and valuable map of the geology of Piedmont; Professor Savi of Pisa explained his views, to the same section, respecting the formation of the mountains near Pisa, with which he had long been familiarly acquainted, and M. Pasini exhibited his map of the Alps to the north of Venice, accompanied with a regular series of rock-specimens from numerous localities in that portion of the Alpine district.

On the 8th October the second general meeting of the Scientific Association was held, and papers were read by the President, or by some other distinguished individual, from each section. Great applause was elicited from the audience by the concluding sentence of one of these scientific papers, in which the author, who was an eminent Grecian geologist, stated, that "in ancient times the inhabitants of Italy were accustomed to visit Greece in search of knowledge, but that, at the present time, the inhabitants of Greece were in the habit of coming to Italy to acquire additional knowledge."

A magnificent dinner was given by the Grand Duke of Tuscany to all the scientific members of the Association on the 10th October, and about three hundred philosophers sat down to this banquet, at which the Governor of the city of Pisa presided. After dinner, the health of Leopold II., the Grand Duke, was drunk with evident gratification, and verses were recited in well-merited praise of his liberality.

On the 14th October, the sectional meetings terminated, and on the 15th October, the last general meeting of the Association was held, when the Grand Duke of Tuscany took his seat at the right hand of the President. Reports were read from each of the sections; the laws of the Association, drawn up by the committee of the presidents of the sections, which formed the governing body of the Association, were declared; and it was announced that the next meeting of the Italian Association would be held, in 1840, at Turin, and that the meeting in the following year, 1841, would take place at Florence.

LXXVIII. *Intelligence and Miscellaneous Articles.*

CHLOROSULPHURET OF MERCURY.

M. CAPITAINE prepares this compound (which he calls a chlorohyposulphite of mercury) by mixing accurately ninety-four parts of bichloride of mercury and six parts of sulphur. The mixture is to be put into a platina capsule, covered with a funnel, and very moderately heated; an efflorescence rises from all the points of the surface and a thick crust is formed; the capsule is to be taken occa-

sionally from the fire to remove the crystals formed, and is to be again heated to procure more of them. This compound may be obtained directly by making a mixture of protochloride of mercury and chloride of sulphur in sufficient quantities to form a thin paste. After twenty-four hours' digestion in a close vessel, it is to be gently heated; the excess of chloride of sulphur is dissipated; the matter fuses, becomes red, and sublimes; this compound may also be formed by the action of sulphuret of arsenic upon bichloride of mercury.

Chlorosulphuret of mercury is of a yellowish white colour; the crystals are exactly similar in form to those of bichloride of mercury; when exposed to the action of heat, they fuse into a brown liquid, capable of boiling and volatilization without undergoing any change. This compound is immediately decomposed by water, which converts it into bichloride of mercury, and sulphur, which precipitates in the state of a granular powder. This ready decomposition effected by water, renders the analysis very easy; for it is sufficient to take a known weight of it, to boil it for some time in water, and to determine the quantity of sulphur deposited, which being subtracted from the weight employed, the difference is bichloride of mercury. Having, however, found this method not to be quite free from objection, the relative proportions of the elements were determined by direct means. The mercury was reduced to the metallic state, the sulphur was obtained uncombined, or as sulphate of barytes, and the chlorine in combination with silver.

A known weight of the compound was passed in vapour over iron filings heated to redness. The mercury was condensed at the end of the tube, and 100 parts yielded 69 of it; in another experiment, in which the mercury was converted into sulphuret, the same result was obtained.

In order to determine the sulphur, 100 parts were heated to redness with soda and nitre. The product of the operation dissolved in water; the liquor supersaturated with nitric acid, and precipitated by nitrate of barytes, gave 41 of sulphate of barytes = 5·65 of sulphur: by boiling 100 parts in water, so as to effect complete decomposition, 5·70 parts of sulphur were obtained; agreeing very nearly with the above experiment.

In order to determine the chlorine, 100 parts of the substance were dissolved in water; the filtered liquid was treated with sulphuretted hydrogen to precipitate the mercury, and then the excess of this gas being expelled by heat, the chlorine was precipitated by nitrate of silver; the chlorine indicated by this amounted to 24·67.

This salt therefore consists of

Mercury......	69	
Chlorine......	24·67	
Sulphur......	5·65	99·32

or very nearly

One equivalent of	mercury....	202	or 69·67
Two ,,	chlorine....	72	24·82
One ,,	sulphur....	16	5·51
		290	100·

It may therefore be regarded as composed of

One equivalent of chloride of mercury = 238
One „ chloride of sulphur = 52——290

A proof of the probability of this view of its nature is the fact already stated, that it may be prepared by the direct action of these compounds on each other.—*Journal de Pharmacie,* Sept. 1839.

FORMATION OF SULPHURIC ACID. BY M. H. ROSE.

It is well known that sulphuretted hydrogen gas reduces the higher oxides of some metals to a lower state of oxidation. It is generally admitted that this reduction is derived from the formation of water; and the simultaneous separation of sulphur seems to authorise this opinion. A considerable time since, I observed that in some of these cases sulphuric acid was produced, but which could not be detected in others. This fact gave rise to the following experiments:—

Sulphuretted hydrogen gas does not produce sulphuric acid in a cold solution of sesquioxide of iron. If this oxide, recently precipitated, be dissolved in acetic acid, and a great excess of acetic acid be added to the solution, in order to avoid, as much as possible, the production of sulphuret of iron by the current of sulphuretted hydrogen, no sulphuric acid is found in the solution; nor is there any formed if the solution be heated during the introduction of the gas.

If sulphuretted hydrogen gas be passed into a neutral solution of chloride of iron, or acidified by hydrochloric acid, sulphur only is separated, without the production of sulphuric acid. If, however, the solution be heated while the gas is passing into it, sulphuric acid is formed in the solution, the production of which, under these circumstances, seems worthy of observation. It also takes place when a solution is used which is prepared by treating iron with hydrochloric acid and adding nitric acid, and in employing a pure solution of chloride of iron, and which contains no free chlorine, prepared by heating pure iron in chlorine gas. These solutions had hydrochloric acid added to them, in order to prevent the separation of sesquioxide of iron by ebullition. The formation of sulphuric acid cannot therefore be attributed, in these circumstances, to the admixture of a small quantity of nitric acid. When sulphuretted hydrogen is employed to determine the proportion of sesquioxide of iron contained in a mixed solution of sesquioxide and protoxide, by the quantity of sulphur precipitated, the solution must be used cold. —*Journal de Pharmacie,* Aout, 1839.

SULPHURETTED HYDROGEN—ACTION ON SOME SALTS OF POTASH. BY H. ROSE.

When sulphuretted hydrogen gas is passed into a dilute solution of chromate of potash, acidulated with acetic acid, no sulphuric acid is formed; the same is the case when hydrochloric acid is employed instead of the acetic, provided the solution be so dilute that the acid

does not reduce the chromic acid. Sulphur only is separated, but especially in the first case, the separation requires some hours. If, however, the solutions be heated, then a notable quantity of sulphuric acid is formed, sulphur being also always separated, but in small quantity.

A solution of iodate of potash, or of soda even, when cold, is decomposed by sulphuretted hydrogen, and a great quantity of sulphuric acid is formed. It then becomes of a reddish brown colour, on account of the iodine set free, but the ulterior action of the sulphuretted hydrogen decolorates it. The deposit of sulphur, which occurs in this case, is caused by the conversion of the iodine set free into hydriodic acid. The solution reddens litmus paper sensibly after decomposition, and contains sulphuric and hydriodic, but no iodic acid. After the destruction of the sulphuretted hydrogen by a solution of oxide of copper, nitrate of silver produces in the liquor separated from the sulphuret of copper, a precipitate insoluble in ammonia. It is well known that a mixture of free iodine and water is converted by sulphuretted hydrogen into hydriodic acid, accompanied with a deposit of sulphur, and without forming any sulphuric acid. If, however, the mixture be heated during the action of the sulphuretted hydrogen, sulphuric acid is formed in small quantity. A solution of bromate of potash acts with sulphuretted hydrogen like that of iodate of potash. In the cold solution there are produced sulphuric and hydrobromic acids and a deposit of sulphur.

Chlorate of potash, on the contrary, is not decomposed by sulphuretted hydrogen, either cold or even at a boiling heat. Neither sulphuric nor hydrochloric acid is formed, and the liquor does not lose its neutrality. If it assume an opalescent appearance, on account of a trace of sulphur which is separated, it is derived merely from the decomposition of some sulphuretted hydrogen by the air of the atmosphere. If the sulphuretted hydrogen contained in the solution be destroyed by oxide of copper, a salt of silver added to the solution after the separation of the sulphuret of copper, produces no precipitate of chloride of silver. The solution of oxichlorate of potash acts exactly the same as that of the chlorate, with sulphuretted hydrogen.—*Journal de Pharmacie,* Aout, 1839.

WHITENESS OF PRECIPITATED SULPHUR. BY H. ROSE.

It is a well-known fact, that sulphur, which separates from liquids in a state of minute division, has not the usual yellow colour of sulphur, but is whitish or grayish. The cause of this difference of colour has been long a subject of discussion, and it has been generally concluded that it arises from the different states of division. If milk of sulphur and flowers of sulphur be examined by the microscope, the grains of the milk of sulphur are certainly observed to be the smaller of the two; but this is unquestionably not the only cause of the difference between them.

In the opinion of M. Rose it has escaped notice, that the peculiar yellow colour of sulphur is wanting only in those cases in which it

is deposited from solutions containing free sulphuretted hydrogen; the more they contain of this the whiter is the sulphur. It is never whiter than when it is deposited from water saturated with sulphuretted hydrogen, in which the hydrogen of the sulphuretted hydrogen has been gradually oxidized by the contact of the air. If on the contrary finely divided sulphur be precipitated from solutions which do not contain free sulphuretted hydrogen, it has a yellow colour, even when the quantity is small. It is sufficient to decompose a small quantity of a solution of an alkaline hyposulphite by an acid, to be convinced of the truth of this assertion.

White or gray precipitated sulphur, (milk of sulphur) contains a very small quantity of sulphuretted hydrogen, in the state of persulphuret of hydrogen. If it be fused and the small quantity of gas disengaged from the surface of the fused sulphur be conducted by the aid of a current of atmospheric air into a solution of lead, a notable quantity of sulphuret of lead is obtained. M. Rose treated in this way a large quantity of the different modifications of this white sulphur prepared in different modes, and he always obtained the same result.

In melting flowers of sulphur, or roll sulphur, it is true that there is sometimes obtained a little sulphuretted hydrogen; but the quantity of it is so small that it cannot be compared with that disengaged from the milk of sulphur. Water cannot remove from this last-mentioned substance the small quantity of sulphuretted hydrogen which it contains; for all the modifications examined were washed with water till it produced no effect in a solution of lead.—*Journal de Pharmacie*, Aout, 1839.

URIC ACID.

M. Fritzsche has analysed hydrated crystallized uric acid. When the process given by M. Bœteger of preparing uric acid is adopted, and which consists in dissolving pigeons' dung in a solution of borax, and precipitating the uric acid with hydrochloric acid, the acid is procured in much larger crystals when the solution contains a great quantity of organic matter than when it does not contain any. The separation of the uric acid takes place readily, and but a small quantity remains in the liquor. Even this separates on standing, in the form of yellowish brown dendritic crystals, of a line in length, and are hydrated uric acid. This hydrate, when dried at 212° Fahr., loses about 21·52 per cent. of water. It is only these large crystals which are hydrated; whenever it is precipitated from hot dilute solutions the acid is always of this kind; the smaller the crystals are, the more readily they part with a portion of their water at the usual temperature, and it is on this account that they have been so long unknown.—*L'Institut*, No. 304.

CHLORIODIDE OF POTASSIUM. BY M. FILHOL.

When a current of chlorine gas is passed through perfectly neutral iodide of potassium dissolved in twice its weight of water, the first bubbles of the gas communicate a brown colour to the liquor; the

intensity of this colour increases; the liquor soon becomes opake, and iodine is precipitated in a pulverulent state. If the operation be stopped when this deposit ceases to be formed, all the iodine is separated, no sensible quantity of chloride of iodine is formed, and the liquor contains chloride of potassium; if the operation be continued, the iodine redissolves, and the liquor assumes a fine golden colour. Sometimes a little chloriodate of potash precipitates; this happens especially if the iodide is not perfectly neutral, and it is easy to account for it. When all the iodine is dissolved, it is proper to continue the disengagement of the chlorine: fine golden coloured needles soon appear in the liquor, and the quantity increases as the saturation with the chlorine proceeds. When a saturated solution of the iodide is employed, the liquor submitted to the action of the chlorine, is eventually converted into a yellow needle-formed mass of a beautiful appearance; in order that the reaction may be perfect, it is well to heat the liquor slightly, that the tube which conveys the chlorine may not be obstructed.

When the chlorine ceases to be sensibly absorbed, the operation is to be stopped, and then, in order to obtain finer crystals, the bottle may be immersed in water heated from 105° to 125° Fahr.; by this the crystals are redissolved, and recrystallization takes place slowly. The crystals are similar to those obtained by the action of hydrochloric acid on iodate of potash; but the process now described is very convenient for obtaining considerable quantities of this salt.

It appears to be composed of

One equivalent of	bichloride of iodine........	234
One ,,	chloride of potassium	76—310

and when it is heated the perchloride of iodine is expelled and the chloride of potassium remains.

The iodides of ammonium and of magnesium furnished results corresponding to the above; but the iodides of sodium and barium did not yield chlorosalts. Biniodide of mercury was treated in the same manner, and a fine golden-coloured solution was obtained which did not yield crystals.

It will be observed that chlorine acts upon the iodides as it does upon the sulphurets; but the chloride of sulphur combines readily with acid chlorides, whilst the chloride of iodine combines better with the alkaline chlorides.

It ought to be observed, that the salt obtained by the process above described, if it be required very pure, ought to be very quickly separated from the watery liquid, in which it is formed; for if the chloride of iodine is decomposed in iodic and hydrochloric acids, which may happen, as is well known, with great readiness, it may occur that the less solubility of the iodate of potash may determine the decomposition of the alkaline chloride, and the precipitation of a little iodate, for there does exist sufficient hydrochloric acid to prevent the formation of the iodate which occurs rapidly; the salt therefore intended for analysis ought to be separated as soon as it is formed.

The addition of hydrochloric acid is even indispensable to prevent also the precipitation of the salt discovered by Serullas, the crystals of which are readily distinguished by the unassisted eye.—*Journal de Pharmacie,* Aout, 1839.

FALL OF A METEORITE IN MISSOURI, FEBRUARY 13, 1839.

On the afternoon of the 13th of February, 1839, a meteor exploded near the settlement of Little Piney, Missouri, (lat. 37° 55′ N.; lon. 92° 5′ W.) and cast down to the earth one stony mass or more in that vicinity. Mr. Forrest Shepherd, of this city, who was at the time exploring this region in the line of his profession, viz. that of a mineralogical and geological surveyor, hearing of the explosion of the meteor, exerted himself to collect all the circumstances of the occurrence. He subsequently succeeded in obtaining several fragments of one of the stones thrown down by the meteor. Mr. Shepherd has favoured me with an opportunity to examine these fragments, and has also communicated to me the details below related.

The meteor exploded between 3 and 4 o'clock P.M., of the 13th of February, 1839, and although the sky was clear, and the sun of course shining at the time, the meteor was plainly seen by persons in Potosi, Caledonia, and other towns near which it passed. At Caledonia, which is about nine miles south-westerly from Potosi, the meteor passed a little north, and at the latter place, a little to the south of the zenith. Its course was almost precisely to the west. The most eastern spot at which it was seen is about fifteen miles west of St. Genevieve, (or about lat. $37\frac{5}{6}$° N.; lon. 90° W.)—the most western is Little Piney, near which it exploded. To the observers at the latter place, the meteor appeared of the size of a large star. They represent its motion as very slow; but do not state how many seconds it was in sight. We have no data for determining the meteor's size, or velocity, or the inclination of its path to the horizon. The direction of the meteor's motion with regard to that of the earth, was probably such that the velocity of the former would be apparently diminished; and as at Little Piney the meteor must have traversed only a small arc, its motion, to an observer there, would appear quite slow. At the time of the occurrence, Mr. Shepherd was on the western bank of the Mississippi, near St. Mary's landing, and heard a distant report, which he was afterwards inclined to refer to the explosion of this meteor. At Little Piney, Mr. Harrison and others saw the meteor burst in pieces, and in a minute or a minute and a half afterwards, they heard three explosions in quick succession. Some of the inhabitants went in quest of the stones which they supposed had fallen, and finally found a tree which appeared to have been recently injured by the collision of some solid body. Near this tree they discovered (although the ground was covered with three or four inches of snow,) one of the meteoric stones, about as large as a man's head, partly imbedded in the earth; and from the circumstances of its position and appearance, there could be no reasonable doubt that this was the body

which had struck the tree. It is to be hoped that further search will be made for other portions of this meteorite.

The total weight of all the fragments which Mr. S. has brought home, is 973 grains. The specific gravity of one of the small fragments is 3·5 ; but different portions of the stone may vary slightly in this respect, as they may contain more or less of the metallic matter. The resemblance between this meteorite and those of Tennessee, (Silliman's Jour. xvii. 326.) of Georgia, (Ib. xviii. 389,) and of Weston, Conn., is very close, and one might almost imagine that they were all parts of the same original mass. The cohesion of the stone is not great, as it crumbles under a moderate blow. Two of the fragments retain portions of the crust or exterior coating. This is a fifteenth of an inch thick, and bears evidence of intense ignition and partial fusion. It is black, with a wrinkled or cellular surface, and is traversed with seams. The general colour of the interior is an ash-gray. The whole mass is studded with metallic particles, (varying from the size of small shot down to mere points,) and presents numerous rusty spots, and occasional small spheroidal concretions which do not appear to differ in materials from other parts of the stone. The little metallic masses (doubtless of nickeliferous iron) are attracted by the magnet, and are generally permeated by the earthy matter. They are mostly of an iron-white colour, but several are yellow and slightly iridescent. One of these minute masses being removed from the stone, it was by the hammer at once extended into a thin lamina, and was evidently malleable. An analysis may be expected hereafter.

Sept. 25, 1839. E. C. Herrick.

Remark.—Having been familiar with meteorites and examined many of them, I hesitate not to say that I am perfectly assured of the genuine meteoric origin of the fragments described above, even without any reference to the testimony.—*Sen. Ed.* [Dr. B. Silliman] *Silliman's Journal*, vol. xxxvii. p. 385.

ON THE SUPPOSED EXISTENCE OF FLUORIC ACID IN ANIMAL MATTER. BY G. O. REES, M.D., F.G.S.

In the year 1802, Morichini published a paper, in which he declared fluoride of calcium to be an ingredient in human teeth: he was led to examine that substance, from having succeeded in detecting the fluoride in a specimen of fossil ivory. Mons. Gay-Lussac repeated these experiments: and in the 55th volume of the "*Annales de Chimie,*" he states, that the fluoride exists in recent as well as in fossil ivory; and that he had also succeeded in detecting it in the tusks of the wild boar. Fourcroy and Vauquelin subsequently published a memoir in the 57th volume of the "*Annales de Chimie,*" in which they positively denied the existence of fluoride of calcium as an ingredient either in recent ivory or the enamel of teeth: they found it, however, in the fossil ivory of Argenteuil and Lourque, though it did not appear to exist in specimens from Siberia and Layo.

Baron Berzelius has published a paper in the 61st volume of the "*Annales de Chimie*;" in which he states, fluoric acid may be detected in human teeth, bones, and urine; and may be demonstrated, in the latter case, by operating on the precipitate obtained from the excretion by means of lime-water. Since the publication of this paper by Baron Berzelius, the existence of fluoric acid, as a constituent of the animal substances above mentioned, has been acknowledged by chemists generally; and it is mentioned as such in the standard chemical works of the present day.

Having lately been engaged in the analysis of human bone, with more especial reference to those ingredients which have been stated to exist in small proportion, I was led to search particularly for fluoride of calcium. My experiments were made in the usual manner, by trying to obtain the corroding action of fluoric acid on a plate of glass, which was used as a loose cover to a platinum crucible, which contained the substance for examination, mixed with strong sulphuric acid. A gentle heat was applied to the bottom of the containing vessel. In this way, several specimens of human bone (both before and after calcination) were subjected to experiment; but in no instance could I obtain any action upon the glass.

The experiment which the Baron recommends, in order to obtain corrosion from bone-earth, is, to distil equal parts of strong sulphuric acid, and water upon it, until the measure of water is brought over. He states, that the distilied liquor, if evaporated in the glass receiver, will produce a corrosion.—I repeated this experiment using 100 grains of bone-ash, and an ounce of the acid mixture; but could obtain no action on the receiver, by evaporating the distilled liquor; nor was there any corrosion or opacity produced on any part of the apparatus.

During the evaporation of the last portions of the liquor, dense white fumes appeared; and there was some difficulty in vaporizing the whole of it. On neutralizing a portion with ammonia, and testing it with nitrate of silver, a yellow precipitate of phosphate of silver was thrown down. A further examination showed the presence of sulphuric acid, and traces of hydrochloric acid. I was much surprised to find phosphoric acid in this result of aqueous distillation, as the heat had not been urged during the process; for I had considered that acid as of too fixed a nature to volatilize with water at so low a temperature. It appeared to me now, that the presence of phosphoric acid, in this distilled liquor, might be a source of fallacy in the above experiment for establishing the presence of fluoric acid as a constituent of human bone; for it is a well-known fact, that phosphoric acid, if heated on glass of inferior quality till it volatilize, will act upon it with considerable energy*;

* It must be borne in mind, that the fluoric acid acts with facility on every kind of crown or flint glass, however good their quality may be. The supposition that bad glass was used in the experiment is the only means I have of explaining away that which I feel sure is an error on the part of several continental chemists.

and all the animal substances in which fluorine has been said to exist, are particularly rich in phosphoric acid;—thus the ashes of ivory, of human bone, and the enamel of teeth, as also the precipitate obtained from urine by means of lime-water, are all of them composed, in very great part, of phosphate of lime. Mr. Richard Phillips has mentioned (in the Annals of Philosophy, vol. v.), that when the water contained in uranite is driven off from the powdered mineral, a portion of the phosphoric acid is volatilized with it; the heat used being that of a common spirit-lamp. This is the only fact with which I am acquainted (with the exception of my own observation), to show that phosphoric acid will volatilize with water. The heat used in Mr. Phillips's experiment was, most probably, considerably higher than any which I applied. There seems no doubt that phosphoric acid is much more volatile than it has heretofore been supposed.

Having failed in detecting fluoric acid in human bones, I determined on testing for its presence in the enamel of teeth, in recent ivory, and in the precipitate obtained from urine by the addition of lime-water. Two different specimens of ivory (tusks of the elephant) gave no evidence of the presence of fluoric acid, when carefully tested, either before or after calcination; and I was equally unsuccessful with the enamel of human teeth and the precipitate from the urine *.

In these experiments, when I had failed in acting on the glass, I always found that the addition of 0·3 grains of fluoride of calcium to the experiment produced a strong and indelible mark on the surface of the glass test-plate. I mention 0·3 grains, because it will always be found sufficient to produce a most unequivocal corrosion; though I obtained satisfactory results by the addition of a much less quantity. I have had only one opportunity of examining fossil ivory; and in that instance I could not ascertain its locality: on submitting it, however, to the tests used for recent ivory, bone, &c., I obtained immediate action on the glass.

In conclusion, I must express my firm conviction, that fluoride of calcium, as an ingredient in fossil ivory, must be regarded as an extraneous matter, introduced by the partial mineralization of the animal substance;—that no such constituent exists in recent ivory, the enamel of teeth, human bone, or urine;—in fact, that fluoride of calcium should be expunged from the list of the constituents of animal substances.—*Guy's Hospital Reports, Aug.* 1839.

* I was much pleased to observe, that Mr. Pepys, in an analysis of enamel published in Mr. Fox's work on the Teeth, does not mention fluoride of calcium as an ingredient of that substance. This analysis was made by Mr. Pepys in 1833, several years after the fluoride had been declared a constituent of the enamel.

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